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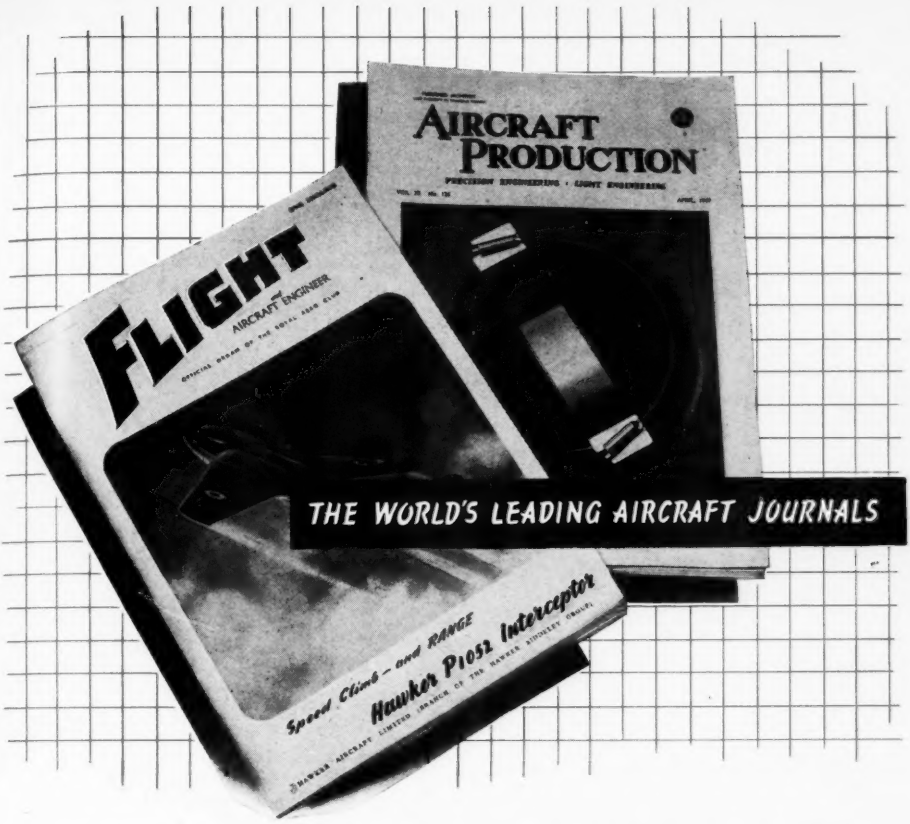

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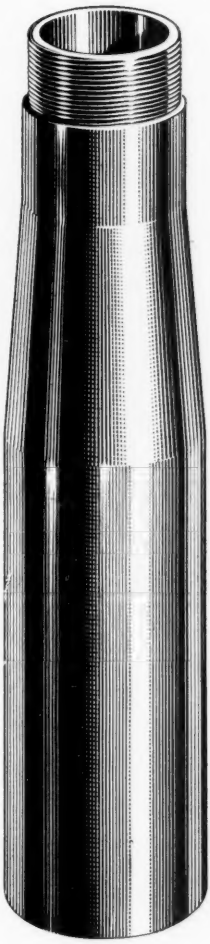

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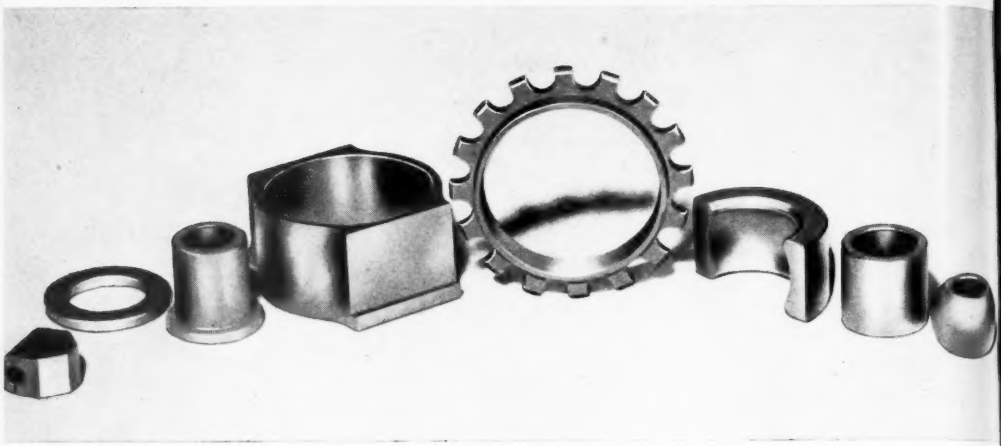

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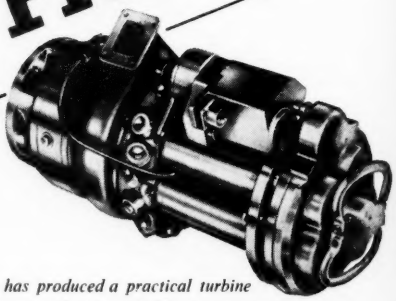

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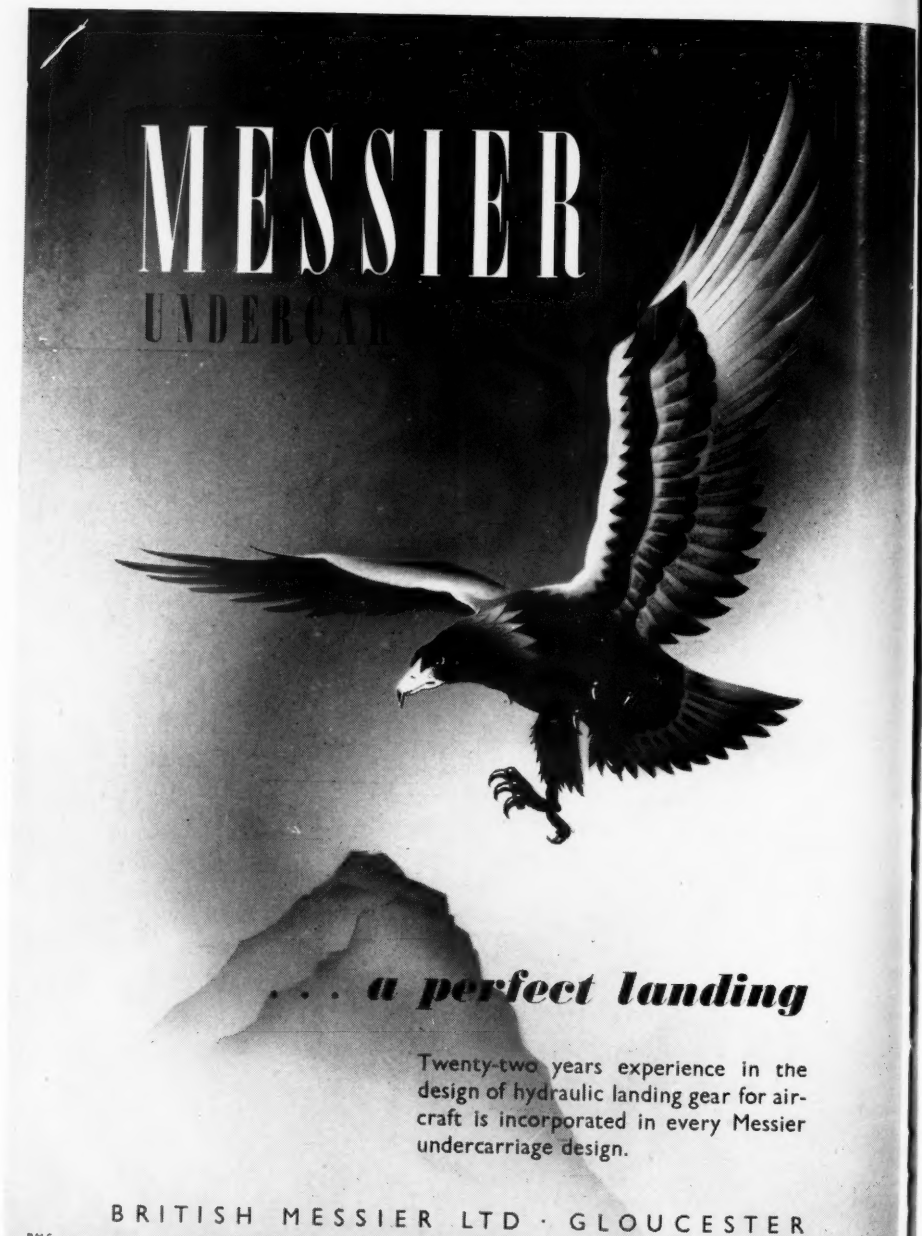
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USE OF WIND TUNNEL MODEL DATA IN AERODYNAMIC DESIGN

by

R. HILLS, B.A., A.F.R.Ae.S.

The 805th Lecture to be read before the Royal Aeronautical Society was given on 19th October 1950 at the Institution of Civil Engineers, Great George Street, London. Major G. P. Bulman, C.B.E., F.R.Ae.S., President of the Society, presided during the first part of the meeting and introduced the Lecturer, Mr. R. Hills, B.A., A.F.R.Ae.S., who had been engaged for some some years on low and high speed tunnel work and was in charge of the No. 2 eleven and a half foot tunnel at the Royal Aircraft Establishment, Farnborough.

Later the Chair was taken by Mr. S. Camm, C.B.E., F.R.Ae.S., Vice-President of the Society.

1. INTRODUCTION

It is nearly twelve years since Miss Bradfield and Mr. Ellis read a paper[1] on this subject to the Royal Aeronautical Society. At that time the Royal Aircraft Establishment was working on the design of a 4,000 h.p. high speed tunnel. Today a large supersonic tunnel is under construction for the Ministry of Supply which will cost over ten times as much to build.

It is proposed to deal in this paper mainly with the *ad hoc* testing of complete models, partly because it is this work which affects the designer directly and partly because it takes a major proportion of tunnel testing time. On some recent advanced designs these *ad hoc* tests have covered a very wide range, as shown by the example of two of these projects where in each case, over a dozen different models and partial models have been made for wind tunnel tests before the prototype has flown. With so much effort going into model work it is important to know the limitations of wind tunnel tests and the accuracy with which flight tests can be predicted.

Unfortunately, reliable comparisons between model and full-scale flight are not very often obtained, either because the design has been changed in important details after the model tests, or else because no detailed quantitative flight measurements have been made. Comparisons between high speed tunnel and flight tests were made in one case in America, by removing the propeller from a typical fighter aircraft, towing the aircraft up as a glider and then releasing it, records being taken in the subsequent dive[2]. In Great Britain fortunately it has not been necessary to go to such lengths to get proper comparisons and from recent flight work several comparisons can be given with model results typical of various aspects of flight work. These results give an overall picture of the value of model work, but it is only by a study of the details of each case that it may be possible to predict what will happen on future designs, where the wing plan forms and section shapes may be very different from existing aircraft. The details of scale effect, especially at transonic and supersonic speeds, are only just beginning to be explored. This work is of vital importance if model data is to be used with confidence. Some aspects of this subject will be discussed later in the paper. First it is proposed to deal with work in tunnels at low speeds.

2. TUNNEL TESTS AT LOW SUBSONIC SPEEDS

In spite of the importance of compressibility effects in many modern designs, the low speed tunnels, where the tunnel speed is less than, say 300 m.p.h., still provide a large proportion of the model results. This is

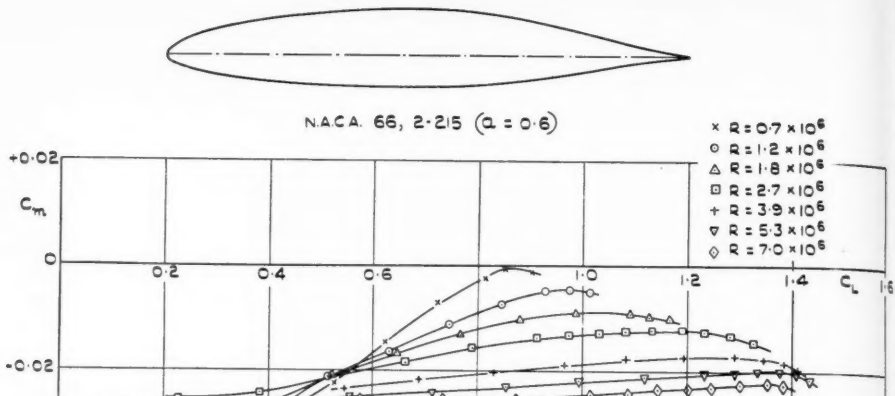


Fig. 1.
Scale effect on pitching moment on low drag section. Compressed Air Tunnel tests.

partly because models can be tested in shorter time and in more detail than they can be in the one large high subsonic speed tunnel in Great Britain and also, because a considerable amount of information which is useful in high speed design can be obtained in a low speed tunnel.

2.1. Longitudinal stability and slipstream tests

The normal low speed complete model test is made at a Reynolds number of the order of one million, although when slipstream has to be represented the limitation of the power of the motors usually reduces R to half a million. The comprehensive series of aerofoil tests made by the National Advisory Committee for Aeronautics[3, 4], covering a range of Reynolds numbers from these values up to very nearly full scale, have shown that for aerofoil sections with their maximum thickness forward of about 40 per cent. chord, scale effects on longitudinal characteristics are small, except near the stall. Recent tests have demonstrated that for aerofoils with their maximum thickness farther back, this is no longer true.

Figure 1 shows, for example, some results from compressed air tunnel tests on an aerofoil which has its maximum thickness at about 45 per cent. of the chord. The kink in the pitching moment curve at a lift coefficient of 0.4 is not removed until a Reynolds number of 7×10^6 is reached. This behaviour is probably due to a separation at the rear of the aerofoil where there is a steep adverse pressure gradient. At low Reynolds number the boundary layer may remain laminar up to this adverse pressure gradient and then separate without forming a normal turbulent boundary layer. As the Reynolds number increases the extent of this laminar separation decreases, reducing the pitching moment instability above $C_L = 0.4$ and reducing the drag coefficient. At about $R = 2 \times 10^6$ the drag at low lift coefficient begins to increase, indicating that transition is moving forward and that the boundary layer has become turbulent before the separation point. In this case, however, the adverse pressure gradient at $R = 2.7 \times 10^6$ say, is strong enough to cause a turbulent separation at moderate values of C_L. This turbulent separation is smaller in extent than the laminar one and decreases with increase of R until it has almost vanished at about $R = 7 \times 10^6$.

This type of scale effect is often difficult to deal with in low Reynolds number tests. In some cases the normal technique of avoiding laminar separation by fixing transition forward by means of a thread can be applied with success, but if the pressure gradient is very large at the rear of the aerofoil, the addition of a thread may result in the laminar separation being changed to a turbulent one

and the overall results will look very similar. Tests at a high Reynolds number are then the only satisfactory way of getting any reliable results. Fortunately thick aerofoils of this so-called low drag type are not being used much on current designs. It should be remarked that the aerofoil section shown in Fig. 1 appears to have quite satisfactory characteristics in flight as it has been used on several American aircraft, including the King Cobra.

Apart from this specific case of large scale effect due to aerofoil section shape, the main difficulty in interpreting low speed tunnel tests is in deciding whether flow breakdown due to interference effects from fuselage, nacelles, drop tanks, and so on, are likely to be present at full-scale Reynolds numbers. The flow is usually more liable to breakdown at the model scale but if the shape can be improved to prevent the flow breakdown on the model, the modification will certainly improve the full-scale design and make it less touchy to the effect of roughness, leaks, and so forth. Experience is the best guide in any particular case and it is impossible to give any general rule.

As an example of the excellent agreement which can be obtained between model and full scale, the trim curves, flaps up and down, for the Brabazon I are shown in Fig. 2. Similar good agreement is shown by tests on the Dakota (Fig. 3), including in this case the effect of engine power. Tests on a four-engined aircraft (Fig. 4) do not agree quite so well, although there is only a small difference with flaps up. With flaps down the undercarriage was down full scale but was not represented on the model. This is normal practice as the effects of an undercarriage are liable to large scale effects and the change of trim full scale, due to undercarriage, is only expected to be small. Flight tests to check the undercarriage effect on trim have not been made, but it seems probable that this is small. If this is so, it is likely that the difference between model and full scale is due to scale effect on some interference between nacelles and flaps, rather than any direct scale effect on the pitching moments produced by the flaps.

2.2. Maximum Lift Tests

Increasing Reynolds number can have a large effect on maximum lift and the normal low speed tunnel test at $R = 1 \times 10^6$ does not provide a very useful comparison with flight results. A number of complete models have now been tested in the R.A.E. 10 ft. × 7 ft. tunnel over a Reynolds number range nearly up to full-scale values and Figs. 5 and 6 show these results compared with some R.A.E. flight tests. The only major disagreement is the Meteor with flaps up. Part of this difference may be due to the fact that in this early test in the tunnel the Mach number was not always kept low enough and at $R = 5 \times 10^6$, flaps up, it was 0.2 compared with 0.12 in flight. This difference in Mach number might cause a drop in $C_{L\max}$ of about 0.05. The other tests are at Mach numbers nearer the flight values. It should be noted that,

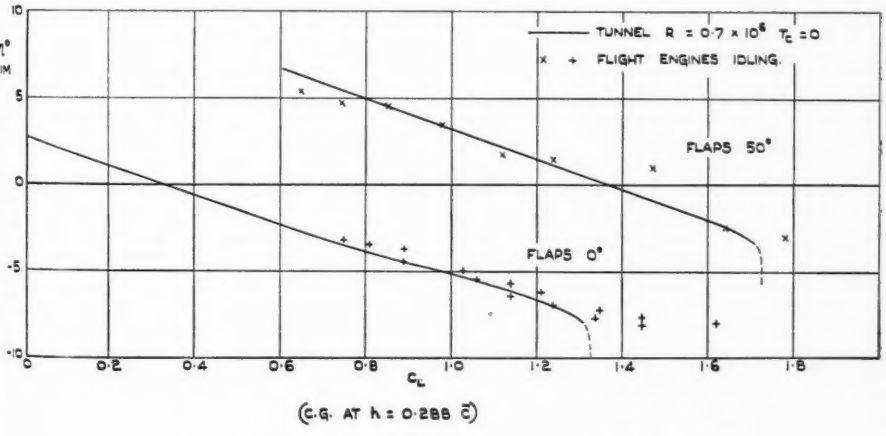


Fig. 2.
Comparison of flight and tunnel trim curves—Brabazon I.

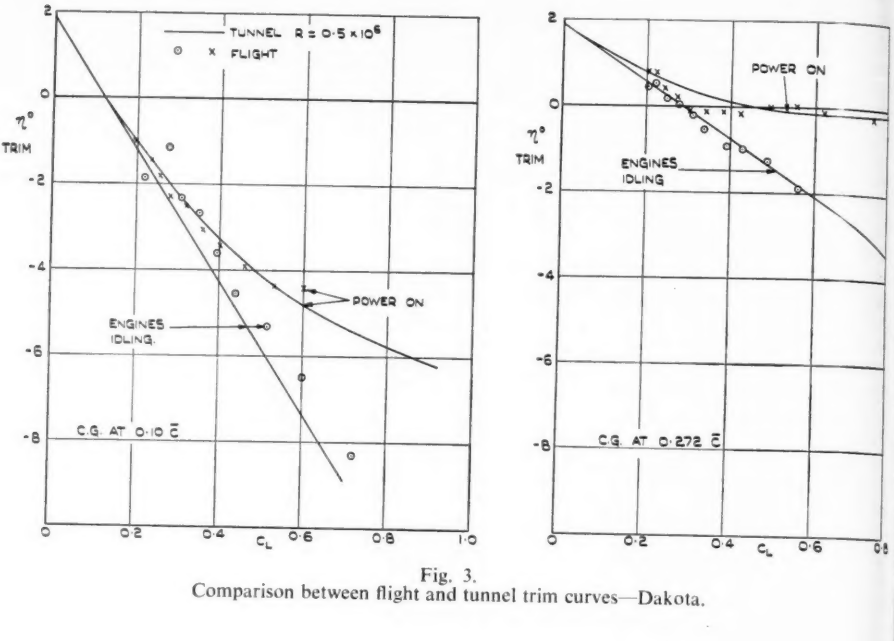


Fig. 3.
Comparison between flight and tunnel trim curves—Dakota.

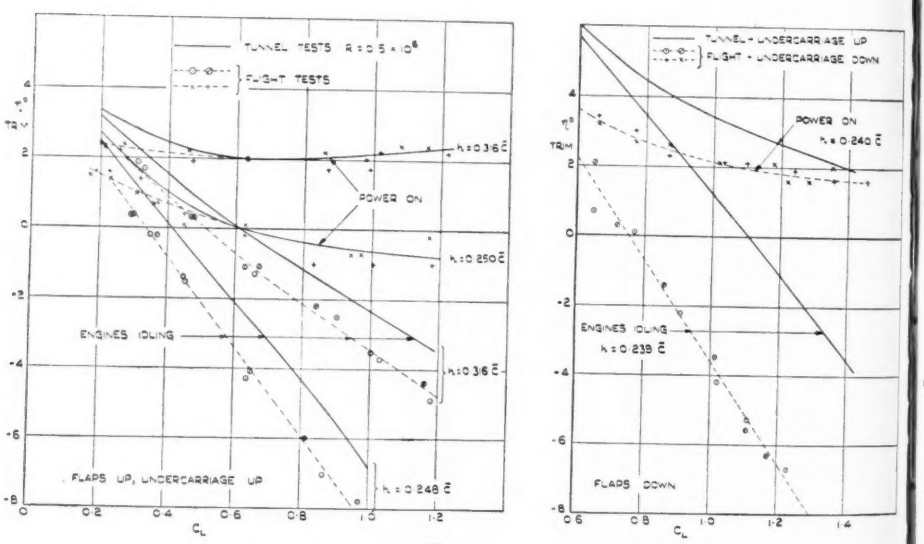


Fig. 4.
Comparison of flight and tunnel trim curves (four-engined aircraft).

particularly on those aircraft with leading edge wing intakes or nacelles (Vampire, Meteor and Mosquito), the scale effect is not as large as might be expected from tests on plain aerofoils, so that the optimistic scale effect increment usually assumed is often not forthcoming.

This suggests that interference effects of nacelles, and such like, on maximum lift, persist to high Reynolds numbers and if they are found in low speed tunnel tests, it is advisable to try to improve the design and not just hope that scale effect will clear up matters. As already mentioned the model is likely to be more sensitive than the full-scale aerofoil and this can be illustrated from the stalling tests on the Tudor, where a piano hinge near the leading edge at the root of the wing was found to have a large effect in the tunnel at $R = 1 \times 10^6$, but no effect full scale.

In order to interpret these tests the mechanism of the boundary layer flow which causes the scale effect must be understood. An interesting technique for visualising the flow has recently been suggested by H. B. Squire which may help in this problem. A thin layer of oil, coloured white to make it visible on the wing, is spread over the upper surface of the wing. When the wind is turned on, the patterns in the oil show something of the conditions in the boundary layer and details of the breakdown of the flow at the stall. Figs. 7 and 8 show examples of this technique used on a Vampire model at $R = 1 \times 10^6$. The white line near the leading edge is produced by a laminar separation

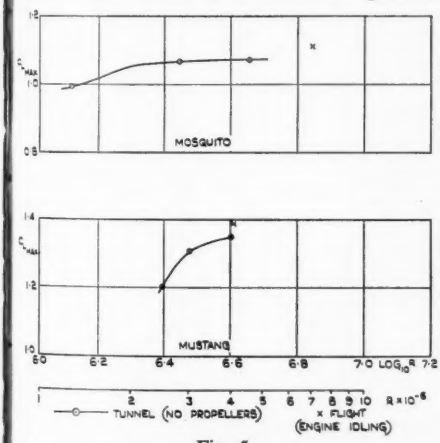


Fig. 5.
Scale effect on maximum lift and comparison with flight (propeller aircraft).

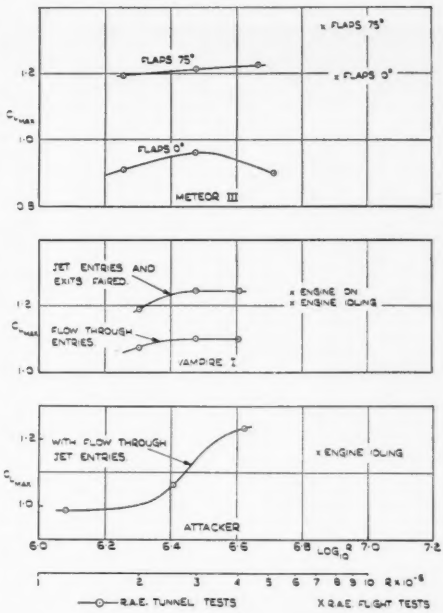
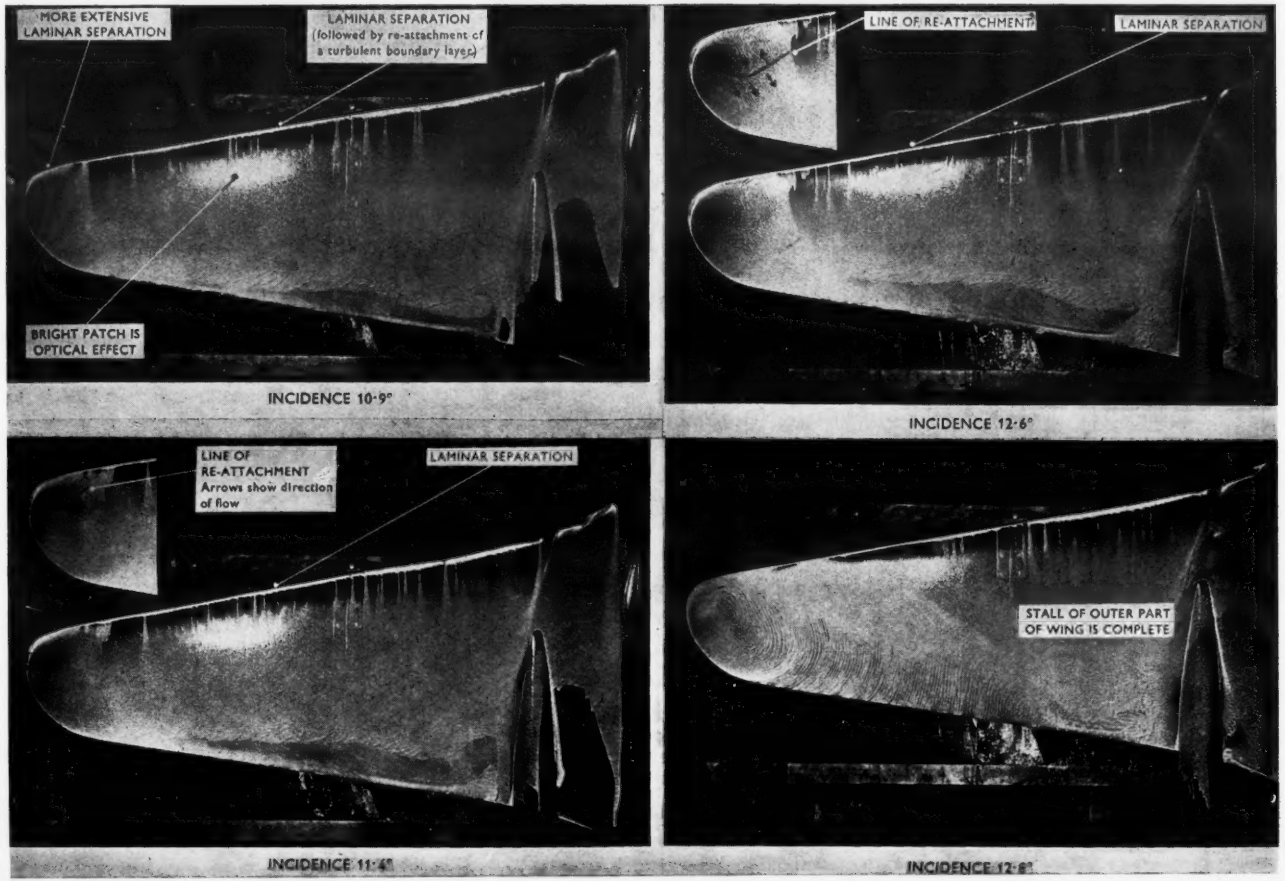


Fig. 6.
Scale effect on maximum lift in tunnel and comparison with flight (jet aircraft).

occurring just after the suction peak at the leading edge. The separation is quickly followed by re-attachment as a turbulent boundary layer, leaving a white line of oil in the separated region. Immediately behind this, the velocity is high and the turbulent boundary layer is thin so that the oil is swept downstream. The stall of the wing starts at the wing tip where the region of laminar separation widens and finally the stall spreads rapidly inboard along the trailing edge. At higher Reynolds numbers, while the laminar separation will still be present, it will extend over a smaller chordwise distance; its final breakdown will be delayed to a higher incidence and may not occur in the same manner. Tests of this type made over a Reynolds number range might provide very useful information to help to interpret model results.

2.3. Swept Wing Tests

Although a considerable amount of model work has been done on swept wings little direct comparison with detailed flight results is so far possible. One aircraft, a tailless glider with 30° sweep, has been extensively


MORE EXTENSIVE LAMINAR SEPARATION
LAMINAR SEPARATION (followed by re-attachment of a turbulent boundary layer)
BRIGHT PATCH IS OPTICAL EFFECT
INCIDENCE 10·9°
LINE OF RE-ATTACHMENT
LAMINAR SEPARATION
INCIDENCE 12·6°
LINE OF RE-ATTACHMENT Arrows show direction of flow
LAMINAR SEPARATION
INCIDENCE 11·4°
STALL OF OUTER PART OF WING IS COMPLETE
INCIDENCE 12·8°

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Figs. 7 and 8.
Photographs of oil traces on a wing near the stall. Vampire port wing.

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tested in both a tunnel and in flight at the R.A.E. This design has comparatively thick wings of conventional section and it can be seen from Fig. 9 that the model tests predicted very accurately the general shape of the trim curves, including the instability at high lift coefficient. The constant difference between the two curves is due to effect of the undercarriage, which was fixed down in flight but not represented on the model.

Figure 10 shows the comparison of l_v and n_v on the same aircraft. While the values of l_v are in reasonable agreement, n_v is much lower full scale than on the model. This is unexpected as n_v depends primarily on the effect of the tip fins and little scale effect would be expected. n_v has also been measured on the D.H.108 and in this case the agreement with model tests at low Reynolds numbers was good. On the other hand the flight tests on the D.H.108 indicated that the instability associated with the tip stall occurred at a higher lift coefficient in flight than in the tunnel.

Scale effect of this type is likely to be a general rule on modern swept-back designs employing relatively thin aerofoil sections and can be illustrated by some C.A.T. results on two delta wings (Fig. 11). On this type of wing planform the tip stall is shown up more clearly by the change in drag than pitching moment and in Fig. 11 the effective profile drag, defined as the drag less the ideal induced drag, is plotted against lift. The rapid increase in profile drag, which occurs when the tip stalls, is delayed to an appreciably higher lift coefficient at higher Reynolds number. This change in lift coefficient, for these particular cases, can be predicted by taking the two-dimensional $C_{L\max}$ for the aerofoil section (R.A.E. 102—10 per cent. t/c along wind) at the appropriate local Reynolds number and, from the theoretical spanwise lift loading, finding the incidence at which a stall might be expected. The points shown in Fig. 11, obtained in this manner, show reasonable agreement with the lift coefficient for which the profile drag rises steeply.

Study of the stalling of a delta wing using the oil technique (Fig. 12), provides an interesting comparison with the stall of the Vampire wing. The stall starts at the tip in a similar way with an extension of the laminar separation, but it spreads inboard along the leading edge rather than along the trailing edge, as in the case of the unswept wing. At a higher Reynolds number the stall occurs at a higher incidence but probably in a similar manner.

The scale effect on the tip stall means that the lateral derivatives at high lift coefficients, as well as the longitudinal ones, will change with Reynolds number. In particular $-l_v$ will tend to be greater in flight than in model tests at low R. Since for swept wings l_v varies linearly with the lift coefficient up to the tip stall, the full-scale value can be estimated fairly well if it is known when the tip stall will occur full scale. It is important therefore, that data should be collected on the scale effect on tip stall on swept wings if normal low Reynolds number model tests are to be used to full advantage.

There is one further aspect of scale effect on swept wings which should be mentioned. In both modern civil transport and bomber designs considerable interest centres on the drag due to lift. Fig. 13 shows plots of the effective profile drag, defined as total drag minus the ideal induced drag against C_L^2 for two swept wings. At low Reynolds number the profile drag increases rapidly with C_L, roughly proportional to C_L^2, but at high Reynolds number the increase is much smaller. The value of K, the ratio of the drag due to lift over the ideal induced drag, is 1.3 at $R=1.3\times10^6$ and 1.1 at $R=8.3\times10^6$ for the delta wing. Some of this difference is due to different transition point movements. At low Reynolds number the transition point is back at zero lift and moves forward on the upper surface as the lift increases, giving a consequent increase in drag.

At higher Reynolds number the transition is farther forward at zero lift and the change with lift is less. This probably does not account for the whole difference and there may well be other boundary layer effects due to sweep coming in at low Reynolds number. Some detailed studies of boundary layers on swept wings are now being made at both the R.A.E. and the National Physical Laboratory and when these are completed more will be understood about these effects. Fortunately they do not seem to cause appreciable changes in other derivatives on swept wings in the range of incidence up to the tip stall.

2.4. Air intakes

This subject has been dealt with in detail by Dr. Seddon in a recent Section lecture[8].

Fig. 7 and 8. Photographs of oil traces on a wing near the stall. Vampire port wing.

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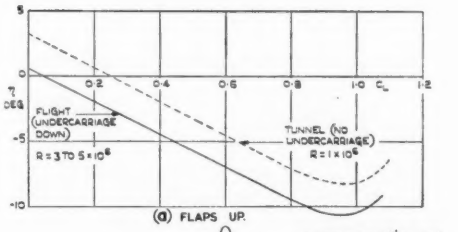


Fig. 9 (*a* and *b*).
Comparisons of flight and tunnel values of elevator angles to trim.
c.g. at $h = 0.247\ \bar{c}$. G.A. Tailless Glider.

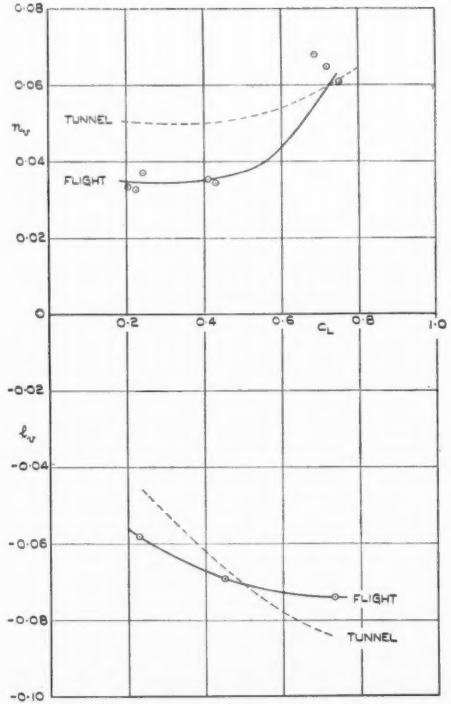


Fig. 10.
Comparison of flight and tunnel measurements of lateral derivatives.
(G.A. Glider, *see* Fig. 9.)

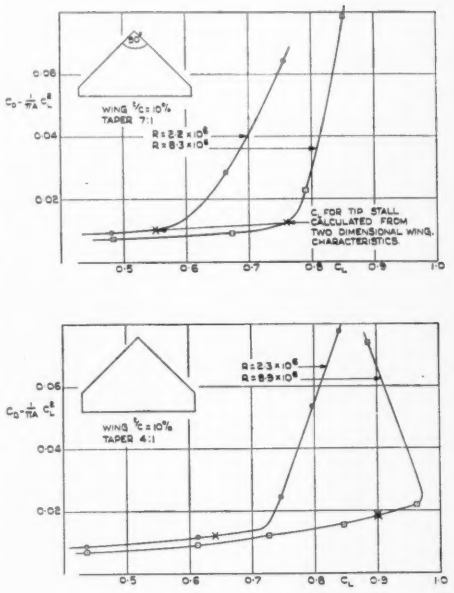
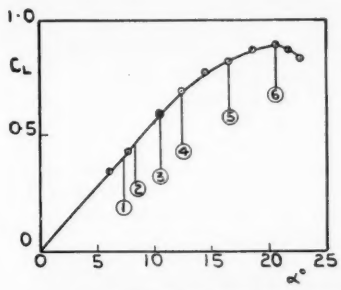
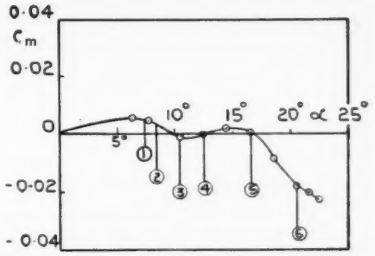

WING ½c = 10% TAPER 7:1
R = 2·2 × 10⁶
R = 8·3 × 10⁶
C_L FOR TIP STALL CALCULATED FROM TWO DIMENSIONAL WING CHARACTERISTICS.
WING ½c = 10% TAPER 4:1
R = 2·3 × 10⁶
R = 8·9 × 10⁶

Fig. 11.
Scale effect on tip stalling.
(Compressed Air Tunnel tests.)

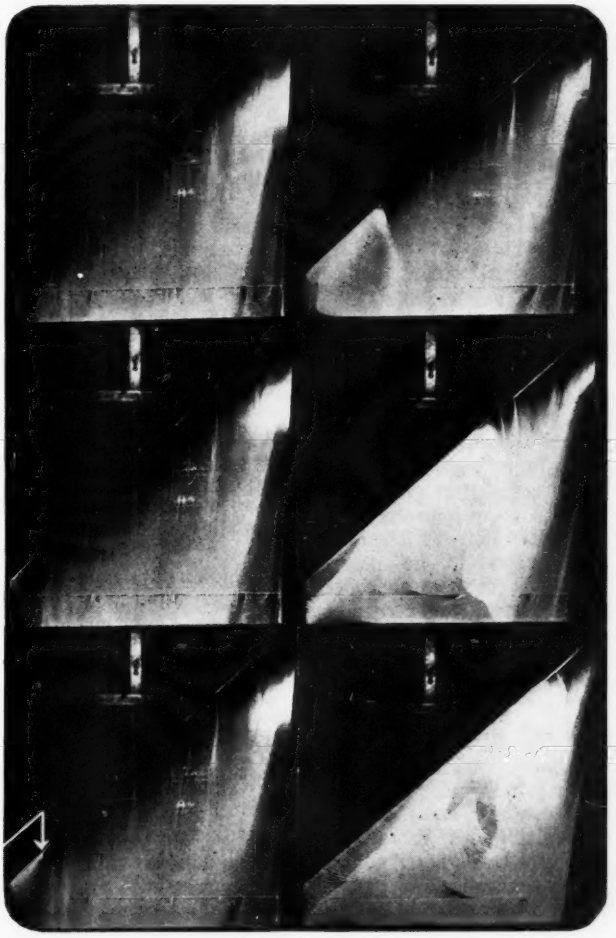




(1)
α = 7.4°
C_L = 0.402

(2)
α = 8.4°
C_L = 0.460

(3)
α = 10.5°
C_L = 0.596
Wing stalled outboard of this point



(4)
α = 12.5°
C_L = 0.693

(5)
α = 16.5°
C_L = 0.805

(6)
α = 20.6°
C_L = 0.890

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Fig. 12.
Photographs of oil traces on a delta wing. Stall of 90° delta, $R = 2.4 \times 10^6$.

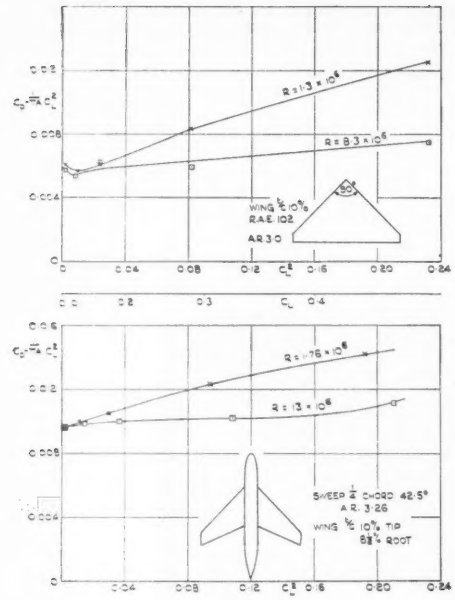


Fig. 13.
Scale effect on profile drag due to lift on swept wings. (Compressed Air Tunnel tests.)

Low speed model tests have shown their value in the design of the details of duct entry shapes and also, by pressure plotting, in predicting compressibility effects. Using comparatively small scale-effect corrections, the full-scale entry loss can probably be estimated fairly accurately. The one exception to this seems to be plenum chamber installations where there have been some unexpected differences between model and full scale. With axial flow compressors coming into more general use, this is not likely to be so important in the future.

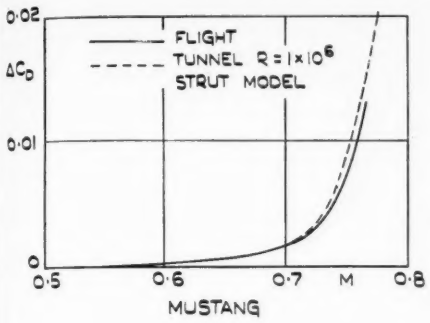
2.5. Hinge moment measurements

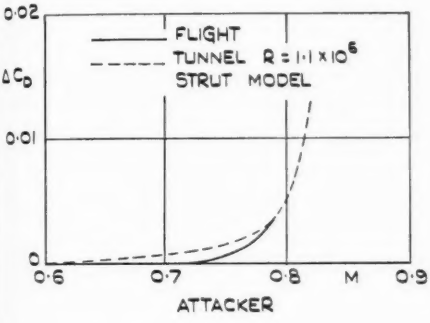
Model measurements of hinge moments on controls are still one of the most difficult to extrapolate to full scale. This is partly because of the difficulty of reproducing exactly the details of the full-scale control, gaps, hinge cut-outs, and so on, and partly because the controls are situated at the rear of the aerofoil where the boundary layer effects are greatest. To avoid some of these effects it is tempting to use a large partial model and it is then found that the tunnel corrections to hinge moments cannot be estimated very accurately. Considerable progress has been made at the N.P.L. recently by L. W. Bryant and his colleagues in elucidating the nature of the boundary layer effects on control hinge moments, at least on two-dimensional aerofoils. This work is going to put both the estimation of hinge moments and the estimation of scale effects on a much firmer footing, but it would need a whole paper to describe these developments.

3. TUNNEL TESTS AT HIGH SUB-SONIC SPEEDS

The development of the technique of testing models in the R.A.E. high speed tunnel was described in 1948 so that it is not necessary to go into the technique of high speed tunnel testing in detail. Over the past seven years a considerable number of models have been tested at the R.A.E. but for the reasons already given it has not been possible to compare very many of the results with flight tests. Some of the few comparisons which can be made are shown in Figs. 14 and 15. Two of the drag measurements (Fig. 14), on the Mustang and the Attacker, are from tunnel tests made with the original technique of supporting the model by struts attached to the under surface of the wings. It was difficult to measure the interference drag due to these struts and the exceptionally good agreement shown may be due in part to an inaccurate estimate of this interference masking the scale effect which might be expected. The results on the Meteor are obtained with a later technique using a sting to support the model from the rear of the fuselage. Unfortunately in this test, one of the first made using this method, the Reynolds number was low. Even so the agreement is good, although it should be noted that agreement of the absolute value of drag at low speed is fortuitous and is due to the fact that the drag of a wing or body with transition far back at low Reynolds number is about the same as the drag with transition forward at high Reynolds number.

Comparisons of the effect of Mach number on longitudinal trim (Fig. 15) also show reasonable agreement. With the Vampire, at the high Mach numbers the trim was changing so rapidly in flight that it could not be measured accurately and the curve is shown dotted. In the tunnel the changes in trim are not quite as large.





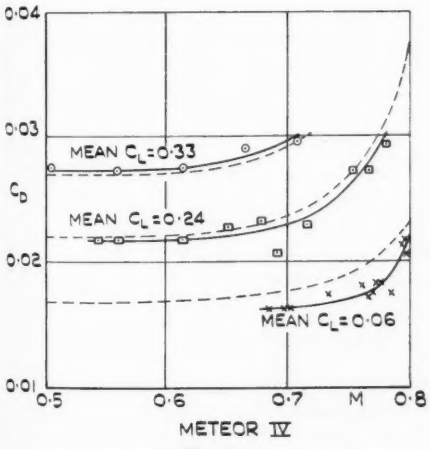


Fig. 14.
Comparison of drag rise with M—flight and tunnel.
——— Flight results (V.T.108), $A = 3.98$ $R = 12$ to 45×10^6.
– – – Tunnel results (E.E.454), $A = 4.95$, $R = 0.43 \times 10^6$.
1/12th scale "Sting" model.

These comparisons suggest that, at least on unswept wings, scale effects at high Mach number are in general small. Tunnel work on one recent design has shown that this is certainly not true for all types of aerofoil section. To illustrate this, Fig. 16 shows the results of some pressure plotting measurements on a wing with a 12 per cent. thick section with one per cent. camber. A large difference in the development of the supersonic region on the upper surface is evident at the three Reynolds numbers of the test and this has an appreciable effect on the variation with Mach number of lift and pitching moment at zero incidence (Fig. 17). At other incidences the variation is similar and the increase in Reynolds number appears to delay the loss of the effects of camber as the Mach number is increased beyond the critical. On a symmetrical aerofoil section the scale effect might be much less. In these tests the transition point was measured by the

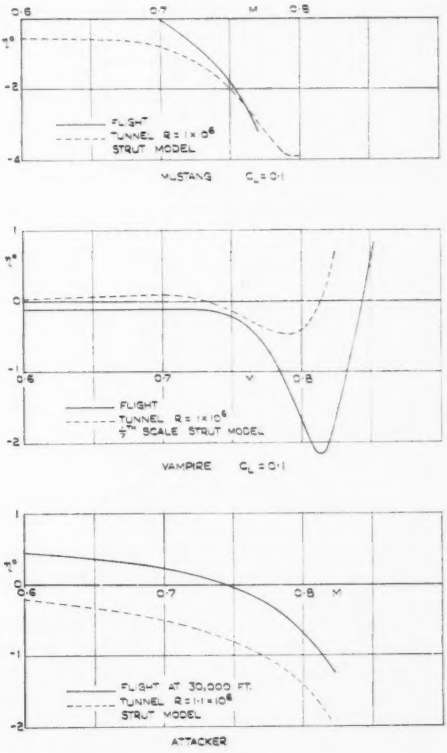


Fig. 15.
Comparison of trim—flight and tunnel.

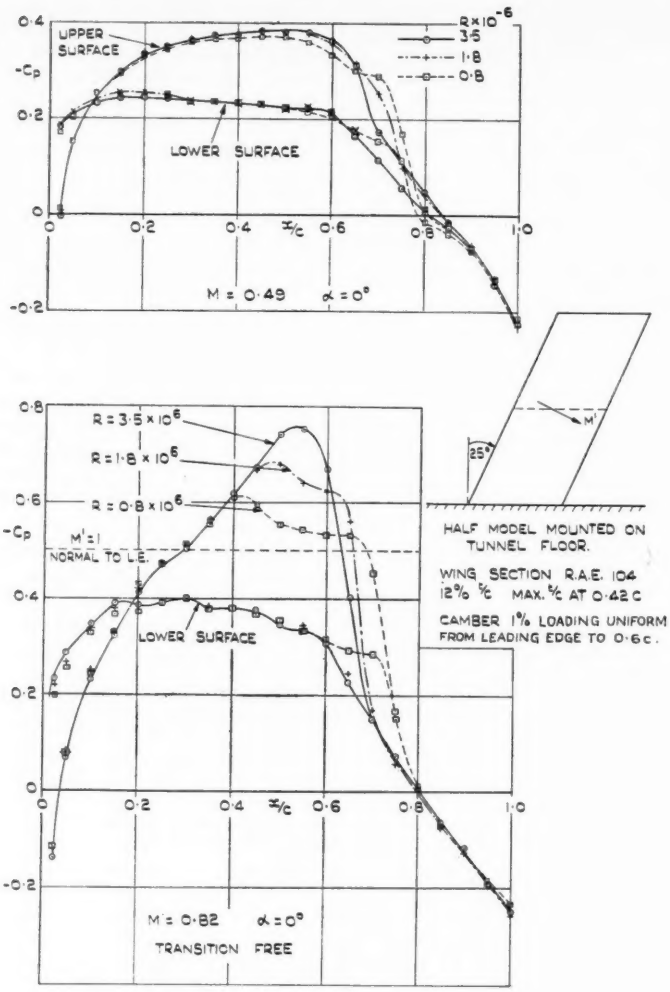


Fig. 16.

Scale effect on pressure distribution at high subsonic M.

sublimation method at $M=0.82$ and appeared to be at about 65 per cent. of the chord in each case.

While this is very reasonable at the highest Reynolds number it is unlikely for the two lower Reynolds numbers. In fact, the shapes of the pressure distribution curves are very similar to those obtained on the wall of a duct by Ackeret in his work on shock wave boundary layer interaction[6]. He showed that at low Reynolds number it was possible, with a laminar boundary layer, to get a λ shock. At $R=0.8\times10^6$, for example, on Fig. 16 there is probably an oblique shock wave starting from the aerofoil surface at about 40 per cent. of the chord; this produces only a small pressure rise but the laminar boundary layer separates. The main normal limb of the shock is at about 65 per cent. of the chord and the separated boundary layer re-attaches as a thick turbulent boundary layer in this region. As the Reynolds

number increases the forward oblique limb of the shock waves moves back until at $R=3.6\times10^6$, the region of separated boundary layer has become very small and the shock wave system is effectively a single normal shock. The sublimation method does not indicate the separated region since the surface velocity is extremely low there and the deposit is not removed, so that transition appears to have taken place at the re-attachment point. It is possible that if the tunnel were run for a very long time the region of separation could be established as the region where sublimation takes longest. Probably, however, it would be much easier to use oil as an indicator along the lines already suggested for low speed $C_{L\max}$ tests. This has not yet been done for this particular model.

A marked contrast in the magnitude of the scale effect is shown if the boundary layer is made turbulent. Fig. 18 shows results using two different methods to obtain a turbulent layer, one with the normal technique of a thread fixed to the wing surface and the second by fixing a wire ahead of the leading edge so that wing is in the turbulent wake produced by the wire. In both cases the scale effect is reduced almost to zero. There is a small difference in these results compared with those with a laminar layer at the highest Reynolds number, the pressure recovery at the trailing edge, for example, being less for the turbulent layer. At higher Mach number this difference becomes slightly larger but it is always considerably less than the scale effects with a laminar boundary layer. It is possible that this difference is due to the thicker boundary layer in the turbulent case

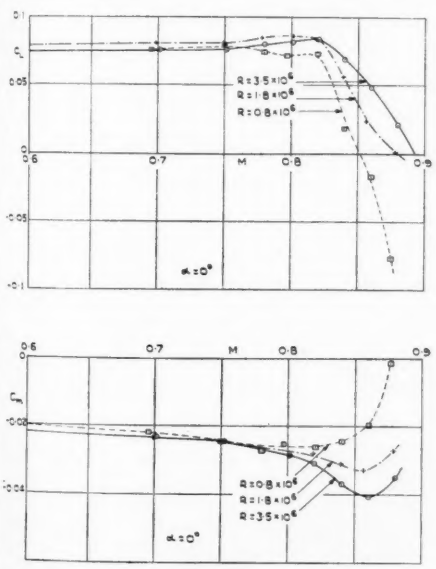


Fig. 17.
Scale effect on lift and pitching moment.
(Wing of Fig. 16.)

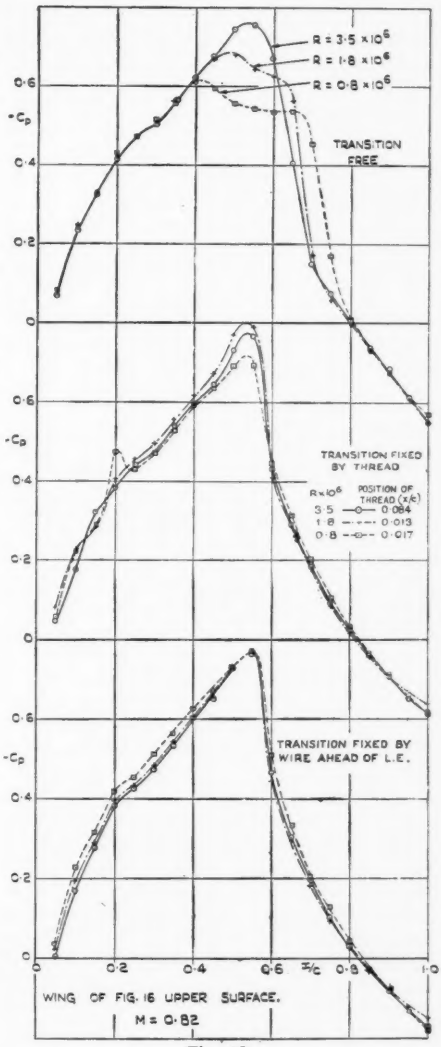


Fig. 18.
Effect of fixing transition on scale effect.

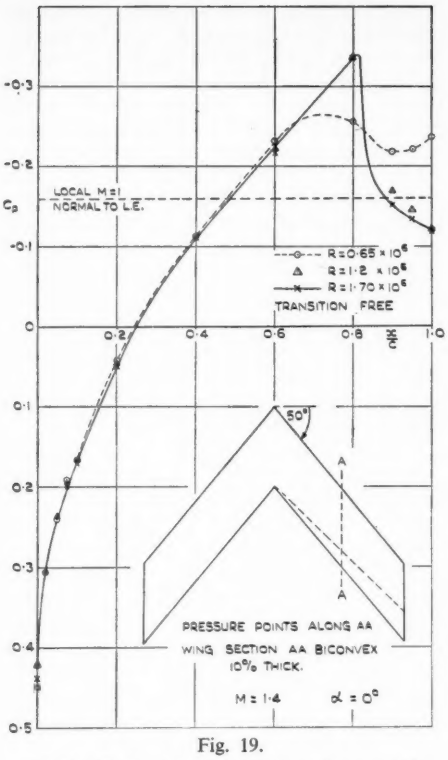


Fig. 19.

Scale effect on swept-back wing at supersonic speed. (R.A.E. 9 in. tunnel test.)

and, if so, it is also possible that the pressure distribution at full-scale Reynolds number, where the boundary layer will be turbulent but thinner, will be similar to that found with a laminar layer at $R = 3.5 \times 10^6$. In any case the results suggest that scale effect will be small as long as the boundary layer on the model can be made turbulent. Unfortunately it may not always be possible to do this easily. The technique of using a wire ahead of the model is not a convenient one for complete model tests, while a thread on the surface has to be put in the region of local subsonic flow, otherwise it will cause a shock wave and alter the pressure distribution on the aerofoil. It is not always possible to do this, especially at high incidence. The N.P.L. are developing an alternative method where small jets of air are let out into the boundary layer and cause transition without starting a shock wave and this method may prove more satisfactory.

Little is known yet about the scale effect on maximum lift, or the tip stalling of swept wings at high Mach numbers. The few comparisons which it has been possible to make suggest that the difference between model tests at about $R = 1 \times 10^6$ and full scale is considerably less than at low Mach numbers. The laminar separation and re-attachment near the leading edge, which as already shown is associated with the breakdown of flow at the stall at low speeds, is no longer present at high Mach numbers. The pressure gradient is favourable up to the shock wave and the stall is associated with the forward movement of this shock and the breakdown of flow behind it with increase of incidence. It is impossible to forecast how much this will be affected by the condition of the boundary layer (*i.e.* whether it is laminar or turbulent) and by Reynolds number until more detailed work has been done on the subject.

It is clear from the flight tests that the important practical limit to the lift coefficient which can be achieved at high speeds in flight is likely to be due to buffeting rather than $C_{L\max}$. This buffeting is usually a direct buffeting on the wing due to a breakdown of the flow there, rather than wake from the wing hitting the tail or fin. On the model, silk tufts, either stuck on the surface or raised on pins, can be used to indicate these breakdowns. In the R.A.E. high speed tunnel, tufts stuck on the surface with self-adhesive tape running spanwise along the wing have been used. These tufts do not appear to alter the lift and pitching moments on the model much and they give a very clear indication of a thickened boundary layer or a breakdown of the flow. Few detailed correlations with full-scale results are possible, but one or two cases have shown that the model gives a fairly reliable indication of full-scale behaviour. One example was an aircraft with tip tanks where the model tests at $R = 1.5 \times 10^6$ showed a flow breakdown just inboard of the tip tanks, at $M = 0.8$ and $C_L = 0.4$. Flight tests confirmed that breakdown did occur at about these values.

Visualisation of the flow is clearly one of the most important aspects of high speed tunnel testing and it is unfortunate that this is rather difficult in the large R.A.E. tunnel, where the working section is in the middle of the pressurised shell and some distance from the operating room. New tunnels now being designed will not suffer from this

defect and this will make use of the various visual techniques much easier. Provided that detailed check can be kept on the boundary layer conditions, high speed tunnel model tests at about $R = 1 \times 10^6$ appear to give results reasonably representative of full-scale conditions, although there is no doubt that exceptions will be found as more data is gradually collected.

4. TUNNEL TESTS AT SUPERSONIC SPEEDS

So far most of the supersonic tunnel work in Great Britain has been done on two-dimensional aerofoils or projectiles and the problem of testing complete models is just beginning to be tackled. In the small 9 or 10 in. diameter tunnel the technique difficulties are large. For example, in one such test which has been planned, it is necessary to design a three-component strain gauge balance to go inside a $\frac{1}{2}$ in. diameter fuselage. This balance has been made but not yet tested. The building of larger tunnels will make the testing of complete models much easier. It is probable that the problems, particularly on swept wing designs, will be very similar to those already discussed in connection with high subsonic speeds.

Figure 19 shows, for example, some pressure-plotting tests on a 50° swept wing at $M = 1.4$ over a range of Reynolds number. The boundary layer was laminar and the effect of increase of Reynolds number is very similar to that shown in Fig. 16 at subsonic speeds. Oil let out from a hole near the leading edge travelled back along wind until it reached the pressure minimum and then moved outboard in a direction parallel to the leading edge. At the low Reynolds number it moved outboard as a broad band indicating a region of laminar separation and re-attachment, but at the highest Reynolds number the band of outflow was very narrow. No tests have been made yet on this model with turbulent boundary layers, but similar results to those obtained at subsonic speeds might be expected. Visual methods of studying the boundary layer therefore will be just as important in supersonic tests as in subsonic tests.

In the future there will be a large extension of supersonic tunnel work, both on aircraft and on guided missiles. One of the most interesting developments will be the effort to

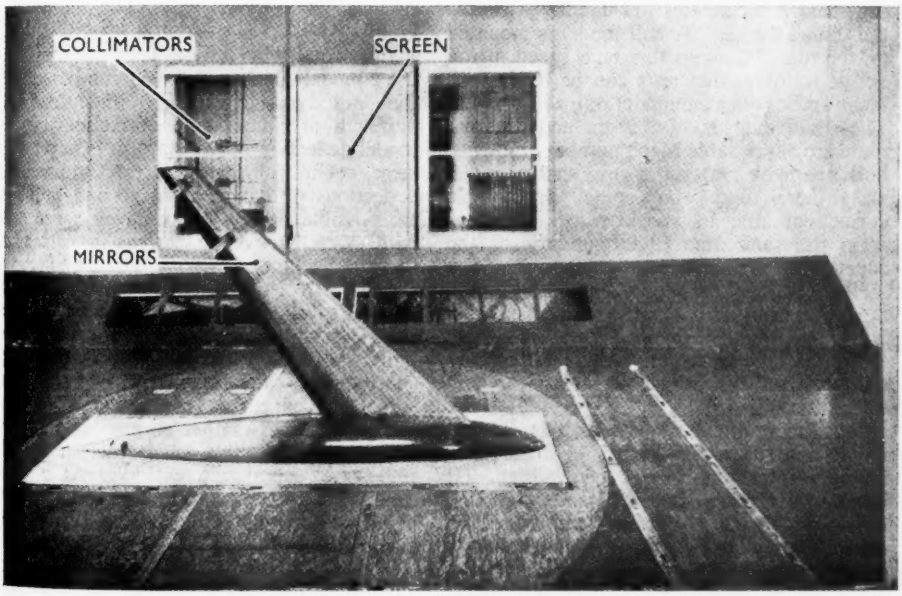


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Fig. 20.
Flexible model mounted in tunnel.

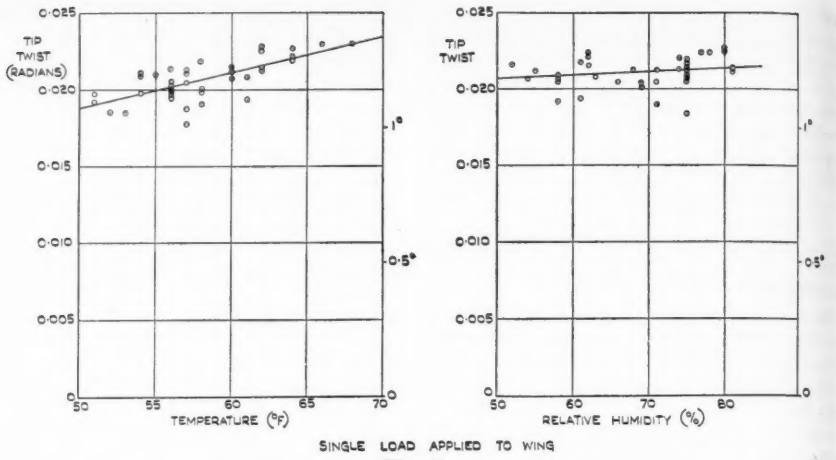

Fig. 21.

Effect of air temperature and humidity on stiffness of flexible model of 45° swept wing.

find methods of using a tunnel to test models in the transonic range between, say $M=0.95$ to 1.2. Up to the present the only method of doing this is by using the transonic "bump" technique. The floor of the tunnel is shaped like the top surface of an aerofoil so that the flow near it accelerates through $M=1$ before the tunnel chokes. A half model is mounted on the tunnel floor in this high speed region and it is found that tests can be made with mean speeds over the model of up to $M=1.2$. The disadvantages of this method are that there are appreciable Mach number gradients, both spanwise and chordwise, on the model and there may be some appreciable corrections due to shock wave reflections from the sonic line (the boundary of the supersonic region near the bump). The method can be used only on a very small model so that it can only be applied in a large tunnel with high power if a reasonable Reynolds number is to be achieved ($R=1$ million in a 12 ft. tunnel run at atmospheric stagnation).

In spite of these drawbacks this method may prove useful in obtaining some preliminary information on a new design. It has the advantage that the models can be made quickly since they are small and the tests also are simple to make. Unfortunately there is no suitable tunnel for such a technique in Great Britain but the wing-flow technique, which employs exactly the same principle can be used in flight. The model is mounted on the top surface of the wing of the aircraft so that it is in a supersonic velocity region while the aircraft is only travelling at subsonic speeds. The Reynolds number achieved by this means is also about one million, based on the mean chord of the model.

5. AEROELASTIC MODELS

In normal wind tunnel tests the aim is to use a model which is very stiff compared with the aerodynamic forces involved and, although at times some correction due to model deflection is necessary, it is never very large. On full-scale aircraft appreciable deflections of wings, fuselage and tailplane are possible at high indicated air speed and, with swept-back wing designs in particular, these deflections can have a large effect on the stability of the aircraft. These effects are difficult to calculate accurately and if a model can be made to give the correct scale deflections, it can be of considerable assistance in checking the calculations and in finding the aeroelastic effects on a new design. This subject is perhaps rather more structural than aerodynamic, but this new development is of sufficient general interest to give a brief description of some tests made on a flexible half-model in the No. 2 $11\frac{1}{2}$ ft. $\times 8\frac{1}{2}$ ft. tunnel at the R.A.E.

The half-model (Fig. 20) was mounted on a wind tunnel balance below the tunnel floor, the latter acting as a reflection plate. Mirrors were fixed inside the model at various suitable points so that by shining a beam of light

from outside the tunnel, spots of light were produced on a screen mounted on the tunnel wall and measurements of the bending and twist of the wing at various points could be made. The wing was made of cellulose acetate and the detailed structure was representative of a full-scale design. The scale relationship was such that 300 ft./sec. in the tunnel represented 525 m.p.h. equivalent air speed full scale. During the tests a series of check measurements were made from time to time by applying a certain load and noting the deflection. At the same time the humidity and air temperature in the tunnel were recorded. Fig. 21 shows that there is an appreciable scatter on these results up to ± 10 per cent. from the mean value. This scatter is probably due to temperature gradients in the material and to a difference in the humidity between the inside and outside of the model. Typical curves from the balance measurements are shown in Figs. 22 and 23, and they form a fairly consistent set of results. It can be seen that a ± 10 per cent. variation in the aeroelastic effects would not have a very large effect on the absolute magnitude of lift slope $(\partial C_L/\partial C_m)_V$, and so on. Further analysis of these results and comparison with calculations have still to be made.

It is difficult on models made of this type of material to avoid a wavy skin surface and this may affect the aerodynamic loads on the model, particularly on controls. The waviness of the surface of the models which have been made is certainly too large to make it worth while testing them at high subsonic Mach numbers, because the waviness would certainly alter the critical Mach number of the model. At low speeds, where the effect of compressibility is small, the waviness should not produce an appreciable effect on the overall results as long as measurements are not attempted near maximum lift and special care is taken to ensure that the shape of the skin over the trailing edge of control surfaces is correct.

If this technique is to be used as a check on a full-scale design and an attempt is made to reproduce all the details of the full-scale

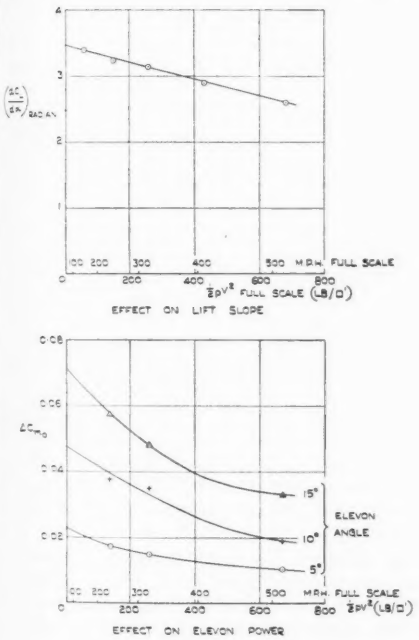

Fig. 22.
Tunnel tests of flexible model of 45° swept wing.

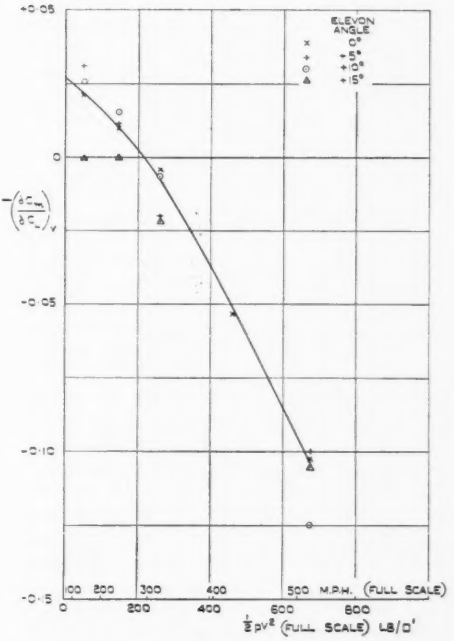

Fig. 23.
Effect of speed on manœuvre margin, flexible model of 45° swept wing.

structure, the model is likely to be expensive and to take a long time to make. It is doubtful whether such a scheme will prove sufficiently attractive to make tests of aeroelastic models of many new designs likely. It is probable that the designer will prefer simpler models which can be built quickly and used to examine some specific problems in the layout. Models of this type can also be used to investigate some structural problems and for flutter tests, so this technique can have quite a wide application.

6. MISCELLANEOUS TYPES OF TUNNELS

6.1. Free-flight and spinning tunnels

The use of a model which can actually be flown in a tunnel sounds a most attractive method of getting information about the flying qualities of a new design. Unfortunately there are snags, the major one being the extremely low Reynolds number, well below values attained in a normal low speed tunnel. Special aerofoil section shapes can be used which have a smaller scale effect than the ones that are normally used, but it is still likely that the various stability derivatives will have values appreciably different from full scale, even at moderate lift coefficients well away from the stall. To use results from a free-flight tunnel, it is desirable therefore to measure the more important of the stability derivatives at the low Reynolds number and compare them with estimates at full-scale Reynolds number. In some cases it is possible to fake the model to give derivative values nearer the full scale. This type of tunnel is probably of most value in trying out some new basic design feature, as for example, the low speed flying behaviour of highly swept wings where there is no great body of systematic data to enable normal stability calculations to be made. A large range of parameters, variations of sweepback, aspect ratio and of some of the lateral derivatives can be covered quickly and the field mapped out so that more detailed calculations can be made later in the most interesting range. The free-flight tunnel is then being used as an alternative to long and laborious preliminary stability calculations, rather that as a direct check of full-scale flying qualities. In this limited field it is probably useful. Few results have been published from the N.A.C.A. free-flight tunnel but those that have been suggest that they have been working along these lines.

Similar remarks apply to spinning tunnels; the Reynolds number is very low and little is known about scale effect at the attitudes of a spin. For unswept aircraft, comparison of tests in the R.A.E. spinning tunnel and flight results have given some practical rules for interpreting model tests. It is not known yet how much these results will have to be modified for the new swept wing designs. The spinning tunnel has been useful in the past in suggesting modifications which have been found to be successful in improving spinning behaviour in flight, but it has not proved by any means infallible and there is still much to learn about the use of such a tunnel.

6.2. Stability tunnels

The N.A.C.A. have built two novel types of tunnel for measurements of some of the rotary stability derivatives which cannot be measured easily in a normal tunnel. In one, the rolling flow tunnel, vanes are arranged to rotate the flow so that it represents the flow around a rotating model. Stability derivatives due to roll can then be measured easily. Comparison with tests made on a typical model using the normal technique of rotating the model, show extremely good agreement[7]. The main advantage of this type of tunnel is that it is fairly easy to apply to a circular tunnel and does away with the need for the rather complicated balance required if a model is to be rotated in a normal tunnel.

The second tunnel is known as the curved flow tunnel. The walls of the working section are curved and the velocity across the tunnel is graded by means of screens, so that the flow is the same as that round a model on a whirling arm. Derivatives due to rate of yaw and rate of pitch can then be measured. In Great Britain efforts have been concentrated on building a whirling arm to get these stability derivatives, but the work on this project at the N.P.L. has shown that it is very difficult indeed to get rid of interference effects and it is possible that the use of a curved flow tunnel might be rather easier. There are some detailed differences in the flow in the two cases, however, and these need further investigation.

In most of the stability problems which are found in flight it is the oscillatory derivatives measured in oscillating motion which are important and more effort should probably be devoted to the measurement of these rather than in trying to measure the derivatives due to steady rates of pitch and

yaw in turns and pull-outs. These oscillatory derivatives can be measured in a normal tunnel by techniques that were developed in the early days of wind tunnel work. Such tests can now be made at considerably higher Reynolds numbers and higher speeds and the accuracy can be improved by using such devices as air lubricated bearings and electronic methods of recording. As the effect of compressibility on these derivatives is practically unknown, especially on swept wings, probably quite a lot of work will be done on this subject in the near future.

7. CONCLUSIONS

This paper has outlined what a designer can expect from model tests and has pointed out some of the pitfalls. In the past the main aircraft firms have had only low speed tunnels at the best but some of them are now just beginning to get reasonable sized high speed tunnels. It has been shown that many results from such a tunnel at, say $R=1$ to 2 millions, will probably be applicable to full scale with little scale effect correction, provided that laminar boundary layer separations can be avoided. Means of visualising the flow, particularly in the boundary layer, will be most important in such a tunnel and fortunately in the tunnels which are being built it will be much easier to do this than in the R.A.E. high speed tunnel. One point which should be emphasised is that in these high speed tunnels the models will be comparatively small if reasonably high choking Mach numbers are to be reached, and they will have to be made accurately if reliable results are to be obtained. There is much to learn still about compressibility effects, especially on swept wing designs and there is no doubt that the behaviour of full-scale aircraft will not always work out as predicted from model tests. A good beginning has been made and it seems that the large amount of effort that is put into wind tunnel model testing will be justified.

In addition to the model test work on actual aircraft designs, wind tunnels have to deal with the need for both systematic data and fundamental work where we endeavour to extend our knowledge of the details of aerodynamics. No attempt has been made to discuss this type of work, but it is of great importance if model tests are to be used correctly and if designers are to be provided with information for future designs. The number of wind tunnels in Great Britain is limited and consequently there is always competition between the *ad hoc* model tests and the other work. Perhaps in the future as more tunnels are built things will be easier. There will always be plenty of model work to keep any number of tunnels busy and if the results from these tests are used intelligently, they should provide a valuable contribution to the improvement of aircraft design.

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DISCUSSION

Professor E. J. Richards (Southampton University, *Fellow*): As one who had been engaged in the Industry until very recently, he wished to pay tribute to the co-operation they had always enjoyed with Mr. Hills and his colleagues at Farnborough. It might have been Socrates or it might have been an equally learned Chief Designer in Great Britain who had said that he could go on designing aeroplanes all day if he had not also to build them and to make them fly; and there was a lot of wisdom in that. The business of having to "carry the can" was a very large responsibility, and it was sometimes felt that it was not appreciated by those who had just to give advice and had not actually to build. That was not so in the case of Mr. Hills.

He was disappointed that more had not been said about shock wave indication; it would help greatly to know the shock wave pattern along the wing and to be able to see it in the wind tunnel. Would Mr. Hills say what was being done in that respect?

It was a pity that Mr. Hills had not dealt with engine cooling and air intake design. He hoped Dr. Seddon's Section lecture would be published as it covered what he believed to be a major advancement in the design of aeroplanes.

He would like to have heard more, also, about the latest developments in transonic technique, for the sooner more was understood about what happened in shock wave formation, the better. He disliked the thought of having to use the rocket technique, shooting things up and hoping to obtain answers from them. It was important to know whether the high speed wind tunnels being put up in the Industry were worth while transonically and that they were right in not spending the money on extensive rocket ranges fit to cope with all the Industry's needs.

Reading the paper, he had been impressed by the extent to which they relied on wind tunnel data. The paper showed that the amount of real theory that they had was limited; it helped them to keep on the right lines, but they relied more and more on wind tunnel data, and must have a better understanding of that data as time progressed. Although the paper covered a wide field, there were many other matters for which wind tunnels were used, such as drop tank design, aerodynamic centres (he did not think there was a really reliable theory on that), stressing loads, engine cooling, air intakes and even that so-called simple item C_{mo} which, to his cost, he had found could vary in the ratio of 3/2 even after wind tunnel tests had been made. The Industry must face the fact that more and better wind tunnels must be built and that it must be much more self-sufficient than it was at present in that respect. Perhaps it was a pity that the Royal Aircraft Establishment had been so co-operative in the past, otherwise the Industry would have had to stand on its own feet to a greater extent and more wind tunnels would have been provided.

They would never reach a stage where they could just put things into a tunnel and say that they did not need to understand the results, but could just use them and design an aeroplane. That emphasised the necessity for the far better training of aeronautical personnel than they were given at the moment. Those who were teaching in the universities had a great duty to perform, and he hoped that as time went on the understanding of wind tunnel work would be much more universal than it was at the moment.

Dr. G. P. Douglas (Royal Aircraft Establishment, *Fellow*): One of the difficulties which arose in tunnel work was caused by the use of over-large models, so that tunnel corrections were not accurately known. Mr. Hills had drawn attention to some remarkable cases of agreement. He had also shown what could be done with a low Reynolds number tunnel in interpreting the results intelligently; but much time and thought had to be devoted to obtaining reliable results from those tunnels. With the new tunnels which were under consideration it was said that they would work at a Reynolds number of about 4 million or so. Mr. Hills had shown that that could be done, by using judgment and by avoiding wing sections which were known to be scale-affected, and obviously it should pay the designer to use sections of the type not so affected in order to obtain reliable tunnel results; Mr. Hills

had shown that even when working to Reynolds numbers down to one million they could obtain quite reasonable results. There were other methods in mind for pushing up the Reynolds number to check those results and that work really needed to be done in parallel.

The ground-launched rocket technique enabled results to be checked fairly simply at considerably higher Reynolds numbers.

In the old days tunnels were used largely for pressure plotting, to find out what were the loads on the aeroplane. Now they had to start from the beginning and get the pressure right so that the flow would be right. A great deal of that work could be done in low speed tunnels, the results being used in conjunction with theory.

Obviously the need to understand scale effect was of major importance. The discrepancy in the maximum lift of the Meteor was probably due just to low Reynolds number trouble at the junction of the nacelle and the wing, which was always a sensitive position, and the trouble apparently cleared up with increased Reynolds number.

He was a little surprised by Mr. Hills' statement, or suggestion, that buffeting was a wing effect and was not due to reaction on the tail. He had always thought that the way in which to avoid buffeting was to have a tailless aeroplane.

He agreed strongly with Professor Richards' point that highly trained staffs were necessary for wind tunnel work.

L. W. Bryant (National Physical Laboratory, *Fellow*): Much had been said on the problem of boundary layers and they all knew that the key to the understanding of scale effects was to be found in knowing what occurred in the boundary layers. At the N.P.L. they had been trying for a good many years to systematise their knowledge of the correlation between boundary conditions and the properties of wing sections. At the same time, work had been progressing on the theory of wing loading in potential flow; it was that progress in the theory which had laid the foundation for attacking the problem of the effects of viscosity on wing properties in three dimensions. It enabled them to tie together their knowledge of sectional characteristics in two-dimensional flow and the finite wing conditions.

With the simplest cases, where surfaces were smooth, without gaps, bumps, and the like, as the designer visualised them before the inevitable details were added, two-dimensional data collected in the form of families of curves could be used. The basic quantity which revealed most of the properties of a wing profile was the slope of the lift curve against incidence; when they knew that they could deduce most of the other derivatives, including the properties of flaps. The lift slope was a function of transition position and of Reynolds number, and could be estimated for any given shape of aerofoil. If they now had the results of model tests at a low Reynolds number they could relate those results to the corresponding section data, correct the section data to the full scale Reynolds number and deduce the full scale quantities required for the aircraft or aircraft parts, so far as wing and tail sections were concerned. The procedure had been worked out for straight wings, and steps were being taken to develop it for swept wings. Meanwhile ways had been suggested for applying those ideas to swept wings which it was believed could be useful until they could be checked by further research. There was already a certain amount of evidence that some success would be achieved.

But if all that were to be valid it was necessary to ensure that they knew sufficient about the boundary layer transitions and separations, if any, in the tests. Laminar separation in particular, characteristic of low speeds of flow, must be suppressed, say by a wire, or the results would be useless.

It was stated in the paper that model measurements of hinge moments on controls were most difficult to extrapolate to full scale, with which he agreed. Mr. Hills had said that that was partly due to the difficulty of reproducing details—gaps, cut-outs, and so on—and partly to the fact that flaps were situated at the rear of the section, where boundary layer effects were greatest. He felt that the second difficulty was best met by making the control surfaces big enough to ensure accuracy of reproduction of contours, hinge positions, and especially the trailing edge, not attempting to include anything but the simple design, the geometrical shapes in fact without local details. If that were done he believed there was good prospect of getting worth-while data which could be interpreted by the process he had described, using two-dimensional standard data for the sections. A partial model on those lines was much to be

preferred to a complete model to an inadequate scale, where they could not rely so well on the dimensions.

The bumps, gaps and other things which he felt made the issue far too complicated to be tackled on the model scale, should be investigated both model and full scale as systematically as possible, so that they could accumulate evidence; an isolated test in a wind tunnel without a careful check on the full scale was usually a waste of time. He was not sure that any bad symptoms cured on the model were necessarily cured on full scale by the same means, in the case of controls. It might be true that a satisfactory control in the tunnel would be so in flight in many cases, if they could assume that break-aways were more likely on the model than on full scale. The research establishments would be fully occupied for a long time in tackling the problems of a straightforward kind, without the gaps and bumps.

At the N.P.L. they had recently come across an instance of scale effect on the tip stall. It was found that one of two types of control was much superior to the other near the stall at a Reynolds number of 0.65 million, whereas at only 1.6 millions the difference had practically disappeared. The controls were tested on the swept wing.

B. P. Laight (The Bristol Aeroplane Co. Ltd., *Assoc. Fellow*): One approach, when starting to design an aeroplane, was to consider how much work had to be put into any aspect of the design, whether it be structural or aerodynamic; generally as little as possible was put in, in the hope that little trouble would be experienced when the aeroplane flew. In dealing with wind tunnel and flight comparisons they had the question of stability, being a linear matter (he called it linear because most of the derivatives were linear) and when testing the complete aeroplane, getting a reasonable Reynolds number generally, it worked out fairly well. The c.g. would not go so far back as had been hoped, and the load had to be pushed forward.

A field inside that low speed area where he considered wind tunnel testing to be deficient was that of control design. The design of controls involved many problems where the angles were small, but he believed that generally good agreement could be secured between tunnel and flight results. Even if good agreement were not secured it did not matter so very much, because if, instead of the aeroplane trimming out at 2° of elevator, it trimmed out at 3°, there were still 3° in hand, and that was all right.

He had been concerned recently with controls at large angles. He believed that in the past, when small aeroplanes were built, it was possible and convenient to effect the final control adjustment on the aeroplanes themselves when they were flown; but that practice was not so convenient in these days, with larger aeroplanes, and they wanted more assurance on that aspect of large angles on control surfaces. There were problems of tab performance, the gaps for tab and controls, and so on. Those problems were all related to the factors which might give very large changes in either direction in the actual aeroplane, and that field of work was one in which there was not a great deal of reliable information.

In order to fill in the gaps, the Bristol Aeroplane Co. had followed through an extensive series of tests on controls of the type he had mentioned. Starting from two-dimensional models, they had done three-dimensional work, for which they were indebted to the R.A.E., which had made the large tunnel available. The controls had actually been flown, but he would not like to draw any overall conclusions from the work yet.

A rudder, operated by a tab and which was expected to give about 28° of control movement, had given about 12° on the first flight. With the help of the R.A.E. they had been able to effect improvements so that now it gave about 18°, and there were still ways whereby it was hoped to put matters right.

Further work in that field would be valuable, and if Mr. Hills would devote some thought to it he would earn the gratitude of the Industry.

J. A. Kirk (*Assoc. Fellow*): Would Mr. Hills give his views on the fixing of transition in low speed tunnel tests? He believed that some work had been done earlier in that connection, but was not very successful so far as the pitching moment was concerned. Inasmuch as Mr. Hills had managed to obtain such good agreement in high speed work, more work at the low speed end would be welcomed.

Most of the figures for flight results showed the $C_{L\max}$ to be of the order of 1.1 or 1.2. If an estimate were made using the scale effect data as given in the N.A.C.A.

reports previously mentioned, they might be tempted to expect a C_{Lmax} as high as 1.6 at the ordinary flight Reynolds number.

Had Mr. Hills any comment to make on this point?

With wind tunnel corrections he believed that the standard constraint correction calculated for straight wings was not applicable to swept-back wings; but he had not seen any published work on that subject and would be glad if Mr. Hills could say anything about it.

Dr. J. Seddon (Royal Aircraft Establishment, *Assoc. Fellow*): As had been pointed out, large discrepancies had occurred between model and full scale results for plenum chamber installations. He believed the reason was not so much in the magnitude of Reynolds number effects in themselves, as in the impracticability of representing the contents of a plenum chamber in sufficiently accurate detail. It was found, in fact, that full scale plenum chamber losses were usually higher than those obtained on a model, which was contrary to the way they would normally expect the Reynolds number effects to go. The experimental department of Rolls-Royce Ltd. had gone farther than the R.A.E. in the matter of model representation, with considerable success. Unfortunately it was in the bad cases that the worst discrepancies were found, those in which, because of insufficient duct area, the air discharged into the plenum chamber in the form of localised high speed jets. Thus they were in a weak position for tackling by means of model tests those cases in which tests were most urgently required. Experience had shown the futility of trying to feed a plenum chamber engine through very small intakes, so that the bad cases were not likely to recur. In more reasonable cases, model tests could provide useful comparative data as between one intake and another, or on the effect of modifications. Thus in the last instance of a model test at the R.A.E., by making comparisons with a known intake they were able to predict the unknown efficiency within three per cent. ram.

Plenum chambers apart, intake loss was a form of drag, and it could now be shown that for a developed intake, *i.e.* one in which flow separations were suppressed, the loss was expressible in terms of an overall effective friction coefficient. On a model, that coefficient appeared to be closely related to that for turbulent flow in a smooth pipe at the Reynolds number of the entry. Scale effects on the model results therefore could be predicted on standard lines. The two principal variations to be considered were the increase in Reynolds number and the change in surface roughness. Those two tended to have opposing effects, and it was fairly safe to assume that in most cases the net scale effect would be comparatively small, amounting to say, one or two per cent. in the ram efficiency of the intake.

The air intake was only one of many features of the modern aircraft to which the method of external pressure plotting for the prediction of compressibility effects was applied. There they must say that the reliability and usefulness of model data obtained at low speeds were not yet properly established. For example, with a wing leading edge intake, the wing had a basic pressure distribution on which the intake distribution was superimposed. The latter was always in the form of a forward region of suction, because the expansion of flow ahead of an intake set the entry lip effectively at positive incidence. Should they keep the two distributions separated so far as possible, even to the extent of increasing the front peak suction, in the hope, which was partially justified, that the effect of that on drag would be negligible? Or should they try to merge the two distributions into one, with perhaps a slightly higher peak but no complications? Only the accumulation of drag data at high speed could resolve that and other allied problems.

N. J. Hancock (Ministry of Supply, *Assoc. Fellow*): Could Mr. Hills give a little more information about the oil film method of test? He would like to know the actual height of the waves of oil that could be seen on the model, and what that represented at full scale. They heard so much about the importance of smoothness and of freedom from the effects of sand grains, fly specks, mosquito corpses, and so on, but he wondered whether on that small scale there might be some interference.

He would also like some enlightenment on statements in the third paragraph of the Introduction to the paper. There it was stated that "Unfortunately, reliable comparisons between model and full scale flight are not very often obtained..." A little later in that paragraph it was stated that comparisons were made by the Americans in one case by removing the propeller from a

typical fighter aircraft; and that "in Great Britain, fortunately, it had not been necessary to go to such lengths to get proper comparisons." Those statements sounded a little contradictory. He would have thought that such truly comparable tests as the Americans had made must be very valuable indeed; would Mr. Hills agree that they were? If he did not agree, how were they to make their comparisons in Great Britain without falling back on intelligent guesswork and the consequent danger of wishful thinking?

N. E. Rowe (British European Airways, *Fellow*): The advance which had been made in the essential business of obtaining good correlation between the facts and the model results so that a very economical means of obtaining design data could be used was most impressive. The agreement obtained was remarkable in many of the phases of design; could the same sort of correlation be expected eventually for the helicopter? The paper had dealt fully with the problem in relation to fixed-wing aicraft; but if they were to create designs of rotating wing aircraft with the comparative certainty now available for fixed-wing designs, considerable study and effort were needed.

D. L. Lambert (Vickers - Armstrongs Ltd.): Until quite recently he believed that it had been generally accepted that the wind tunnel was a satisfactory instrument for predicting C_{M_0}, which was most important from the strength point of view, but as Professor Richards had pointed out there was some doubt about it now.

On the graphs which Mr. Hills had shown relating to the Mustang, the Attacker and the Brabazon, there were considerable differences in the elevator angle for trim as predicted by wind tunnel and as measured full scale.

Those were not, perhaps, direct indications of the actual differences in the value of the C_{M_0}, because possibly the differences in trim angle were due to the downwash in the tunnel not being the same as that in flight. Had any attempt been made to explain the differences, or had they just been accepted as being "differences which did arise"?

S. Camm (Vice-President, *Fellow*): Proposing the vote of thanks to Mr. Hill and to the speakers in the discussion for their contributions, he expressed his appreciation of the painstaking work done by the R.A.E. and the N.P.L. for many years. He had come to realise that the Industry was perhaps somewhat guilty in not having provided more wind tunnel facilities in Great Britain. Unquestionably, if they were to maintain their position in aeronautics in the world, they must have further facilities and the trained staffs that went with them. They could not expect to carry on with the building of prototypes without wind tunnel work, because the alternative of depending on full scale flying was expensive in both money and time and was also dangerous. Therefore he strongly supported Professor Richards' plea for more and better wind tunnel facilities.

D. H. Williams (National Physical Laboratory), *contributed*: Agreement of maximum lift as measured in a wind tunnel with that measured in flight was good except for the Meteor III. It seemed probable that the discrepancy in that case was due to a peculiarity in the maximum lift curve seen for a number of modern sections and shown for the Attacker (Fig. 6) where there was a sudden rise in $C_{L\max}$ at high Reynolds numbers. In some cases that sudden rise only occurred at the highest Reynolds number available in the Compressed Air Tunnel (8×10^6) and for some thin sections the rise had not occurred even then. Hence the discrepancy for the Meteor would be explained if the wing section were one in which the sudden rise took place between $R = 6 \times 10^6$ and 8×10^6.

MR. HILLS' REPLY

Professor Richards: Some development work on the observation of shock wave patterns on swept-back wings in the tunnel had been done at the R.A.E., but it was difficult to devise a method which was fairly easy and quick to use on complete model tests. One of the best indications seemed to be to use an ordinary silk tuft on the surface of the aerofoil. In the region of the shock wave on swept-back wings there was quite a marked outflow, and the pattern could be seen fairly well from the picture obtained; but they did not get an indication until the shock wave had become fairly strong. As stated in the paper, they had been rather handicapped in the R.A.E. high speed tunnel because of the position of the working section; but he hoped that the new tunnels now being built for high speed tests, having the working section right by the operating room, would stimulate the development of a

technique for observing shock waves on complete models.

He could not say much more about the transonic technique. Obviously it was one of the most important aspects of the work at the moment. They were getting correlation between the ground-launched rocket technique for measuring aileron power and drag and the tunnel tests, and on the whole the results were promising. He believed that the ground-launched rockets would prove quite a useful means of checking some of the other transonic techniques, as, for example, the transonic bump or the wing flow technique.

Dr. Douglas: He was stressing buffeting at high Mach number. It might be that in some cases there would be tail buffeting, but he thought that probably the wing buffeting would be more important in most cases.

Mr. Bryant: Next time a firm was doing something really rash in a tunnel he would invite Mr. Bryant to go along and to estimate the tunnel corrections. When there was a 24 ft. chord model in a 24 ft. diameter tunnel, the corrections did become rather difficult, to say the least!

Mr. Bryant had said that there were quite appreciable differences between controls on swept-back wings at low Reynolds numbers and at $1\frac{1}{2}$ million, and it was becoming very clear that on swept-back wings they could not expect good results until they were in the fairly high Reynolds number range, 4 or 5 millions at least was required. He hoped that more tunnels in which such tests could be done would be built in Great Britain.

Mr. Laight: Control tests in general at high angles would certainly be difficult to cover. Work by Mr. Bryant covered what Mr. Laight had called the linear range. He would have thought in general that scale effect would tend, just as it did with maximum lift, to under-estimate the effectiveness of tabs. He supposed that if they could get the control to work on the model and could get the angles they wanted, they were almost bound to get adequate control at full scale. It had been found in the past that hinge gaps, and other details, played a very important part in the efforts to develop controls at the bigger angles, and they had to watch such details when doing model tests.

Mr. Kirk: Concerning the question of fixing transition at low speeds, at the R.A.E. they had found it to be dangerous, especially if they tried to fix the transition forward on a fairly thick aerofoil, because it affected the lift slope appreciably, particularly at high lift coefficient, and reduced the stalling incidence considerably. Apart from putting a wire on some wings where there was a laminar separation at the rear therefore, it was not to be recommended. If there were a laminar separation and they could put the transition wire at the region of 25-40 per cent. chord, it should not have a great effect at high incidence, because the natural transition would move forward ahead of the wire and there would be little effect on the flow.

The low maximum lift coefficients of current jet aircraft certainly called for some comment. The American tunnel measurements of $C_{L\max}$ of 1.6 were made with aerofoils with a fair amount of camber and a fairly large nose radius, *e.g.* N.A.C.A. 23,012 and N.A.C.A. 63-212, and the difference between the flight results and that tunnel value was due, certainly in part, to the use of wing sections on the aircraft which did not have such a high value of $C_{L\max}$. A value of about 1.4 would probably be more representative of the two-dimensional $C_{L\max}$ of a typical wing section used on those aircraft. The difference between that and the average value of about 1.2 for the aircraft probably represented the effect of the finite aspect ratio, fuselage and nacelle interference, roughness and so forth.

Wind tunnel corrections for swept wings had been worked out for a number of cas s and the general conclusion was that the overall constraint correction was very nearly equal to that calculated for a straight wing of similar size and aspect ratio. If the subject were studied in detail, it was found that there was a large variation of the correction across the span of the model with a swept wing and in some cases this might become important and have to be taken into account.

Mr. Hancock: The thickness of the oil film used in the tests had not been measured. Tests were made on the model both with, and without, the oil and it was not possible to detect any difference in the lift or stalling incidence. From this it was assumed that the oil did not affect the flow.

He agreed that the American results on flight tunnel comparisons were valuable but the point of the remarks was that in Great Britain they had been able to get comparisons on jet aircraft which had no propeller

slipstream interference effects which were so difficult to reproduce in a high speed tunnel.

Mr. Rowe: The question of the use of tunnels for helicopters was a difficult one and it was probably impossible to give any definite answer at the present time. Some 24 ft. tunnel tests had been made recently as part of a programme to investigate the use of tunnels for such tests. Unfortunately so far it had not been possible to proceed further with tests in smaller tunnels. With a 24 ft. diameter tunnel it seemed possible to get results which were in good agreement with present-day full scale designs. With a smaller tunnel (*i.e.* 10 ft. to 12 ft.) the Reynolds number on the blade would tend to be below 1×10^6 and some appreciable scale effects might be expected. The scale effects would affect the stalling of the blade and therefore its auto-rotation characteristics and its performance at high forward speed. In between this range the smaller tunnel should still give fairly reliable results and although there was a lot of work to be done on the technique of testing rotating wing aircraft in a tunnel, they would probably find the tunnel just as useful a tool as it had been for fixed-wing aircraft.

Mr. Lambert: He agreed that there were appreciable discrepancies in some cases on the elevator angles to trim as predicted by the wind tunnel and as measured full scale. It had not been possible to explain the reason for those differences. It seemed unlikely that very much of it was due to a difference between model and flight value of C_{m_0}. Scale effect measurements showed no evidence of appreciable scale effects on C_{m_0}. Some of the difference might arise from the difficulty of getting accurate measurements of tail angle, elevator angle, and so on, full scale.

Mr. Williams: Some of the discrepancy in the $C_{L\max}$ comparison for the Meteor might be due to a large scale effect between $R=6$ and 8 millions. It seemed probable that the tunnel result at $R=5 \times 10^6$ was not very reliable, due to too high M, as suggested in the paper, or possibly to some inaccuracies in the shape of the nose of the profile of the wing section on the model.

SOME OPERATIONAL PROBLEMS OF PUBLIC TRANSPORT HELICOPTERS*

by

R. H. WHITBY, D.I.C., A.R.C.S., B.Sc., A.F.R.Ae.S.

1. INTRODUCTION

Throughout the world helicopters are being operated in a variety of roles today and, whereas ten years ago, there was not one type which had got beyond the stage of being an inventor's plaything, several types are now suitable for regular military and civil operations. At present, so far as is known to the author, public transport services using helicopters are confined to charters and scheduled mail services, with the exception of a scheduled passenger service which has been run by the British European Airways Corporation as an operational experiment since June 1950. The factors, both technical and economic, which have so far prevented the large scale introduction of public transport helicopter services, are being progressively overcome and in due course operators will find that manufacturers have made available suitable aircraft. The operators' task will then be to fly these aircraft in a safe and regular manner and give the public the best service possible.

This paper discusses some of the operational problems which require solution before helicopters can reach similar standards of regularity to fixed-wing aircraft, together with an improved standard of convenience on short journeys†. First of all, the progress in, and requirements for, night and blind flight are briefly discussed; then navigation and approach to land under instrument flight conditions and the associated control problems are dealt with; and finally, the requirements for safe operation from restricted sites (which is necessary if the convenience of short distance air travel is to be improved) are considered. It should be emphasised that some of the problems which are discussed, particularly those associated with traffic control, may not arise in a severe form for some time and in the interval other, and possibly simpler, methods of dealing with them will be acceptable.

2. NIGHT AND BLIND FLIGHT

Given an external horizon, night flight presents only minor difficulties and at an early stage helicopter certificates of airworthiness in the United States of America were extended to cover night flight under contact conditions. In the United Kingdom such a concession is of limited value and shortly after helicopters became available in this country, trials were made to determine which instruments were essential for flight without external reference data.

On the fixed-wing aeroplane the primary flying instrument is the artificial horizon. This instrument tells the pilot the attitude of the aircraft to the horizon and therefore at any steady flight speed indicates the flight path of the aircraft. Further, if the aircraft is disturbed and a pitching or rolling motion is shown by the horizon, appropriate correcting action can be taken on the controls by the pilot. Generally speaking, the aircraft possesses stability about all axes and if the disturbance is removed, in due course it will return to the trimmed condition from which it was disturbed; the pilot will normally move the controls in such a sense as to correct a disturbance and his task is eased by the air-

* A Section Lecture given on 14th November 1950—The 808th Lecture to be read before the Society. Mr. Whitby is Assistant to the Controller, Research and Special Development, British European Airways.

† Since an important role of the helicopter may well be as a feeder to fixed-wing services, standards of regularity and punctuality should be at least as good.

craft generally possessing both stick-fixed and stick-free static stability.

The helicopter is fundamentally different from the fixed-wing aeroplane in that at a given forward speed the body attitude changes little with flight path, *i.e.* it is much the same whether the aircraft is climbing, or sinking, or flying level. It was felt, therefore, by some that it would be necessary to incorporate an instrument supplying flight path information such as that which the artificial horizon gives to the pilot of a fixed-wing aeroplane. Further, although possessing static stability, the helicopters first available (Sikorsky R.4 and R.5 or S.51) were dynamically unstable, except over a narrow range of flight speeds, and had to be continually controlled by the pilot; the periods and dampings of the unstable oscillations, however, were not exceptionally critical.

Despite these unpromising characteristics, the standard fixed-wing blind flying panel was installed in helicopters and it was found possible to fly the aircraft blind, at speeds above about 40 m.p.h.* The pilot then operated the controls in such a sense that the body attitude as shown by the artificial horizon was maintained. To keep a steady flight path, constant checks on the rate-of-climb and air speed indicators were then essential, while to maintain a constant heading a gyroscopic direction indicator was found necessary. These instruments, together with the usual engine instruments, constitute the essential instrument flight panel. The rate-of-turn indicator was found to be too sensitive, but the bank bubble was retained as a check, in conjunction with the directional gyro, on the functioning of the artificial horizon in roll; it was also found useful in indicating sideslip. Because of the onerous nature of instrument flight on the S.51, grouping of the instruments to minimise eye movement was found to be particularly important†.

With this arrangement a fair amount of blind flying has been done. Some 360 hours of night flight have been accumulated by the B.E.A. Helicopter Unit alone, of which the greater part was without external reference points (during winter over sparsely populated country in the middle of the night), while seven hours wholly in cloud and more than 50 hours with a safety pilot in simulated blind conditions have been flown. While instrument flight for more than an hour at a stretch is possible on the S.51 in fairly turbulent conditions with a trained pilot, the lack of stability and the heavy control circuit frictions make it tiring[1]. Aircraft characteristics which demand particularly high standards of skill from the pilot and are particularly fatiguing are unsuitable for commercial operation.

A major task of aerodynamic research in the helicopter field is to determine the desirable handling characteristics of helicopters and produce standards for the guidance of designers such as exist for fixed-wing aircraft. Since low air speeds are flown for a very small proportion of the time, and since final approach and landing is likely to be made with the assistance of external visual reference data for some time, an improvement in the handling qualities of the aircraft at moderate and high speed is of more immediate importance. In this connection, the effectiveness of a small tailplane has been demonstrated theoretically[2] and practically[3]; the tandem rotor helicopter is also likely to possess longitudinal stability at cruising or higher speeds[4].

With only single-engined aircraft available at present, airborne lighting aids to illuminate the ground in the event of engine failure are of prime importance. B.E.A. helicopters, therefore, are fitted with two parachute flares and a narrow beam searchlight adjustable in elevation. A wide angle lamp is also fitted to give more general illumination as the ground is approached, and this is also useful in air taxi-ing. Even with twin-engined aircraft, such equipment will continue to be needed to cater for the ultimate emergency when the helicopter cannot reach a prepared landing field.

Another aspect of night operation is the identification of the landing site, particularly in conditions of low visibility. This requires a distinctive light signal from a beacon so designed that the available light energy is concentrated in directions where

* It was found useful to incorporate an adjustable pitch datum in the artificial horizon so that a mean position appropriate to steady flight conditions at a given centre of gravity position and forward speed was indicated.

† It was found that despite careful isolation of the instruments from vibration experienced by the airframe, the artificial horizon life before development of a defect was considerably lower than current on fixed-wing aircraft. To guard against danger arising from failure of the most important artificial horizon, the instrument was duplicated; this is indeed a mandatory requirement for most fixed-wing aircraft.

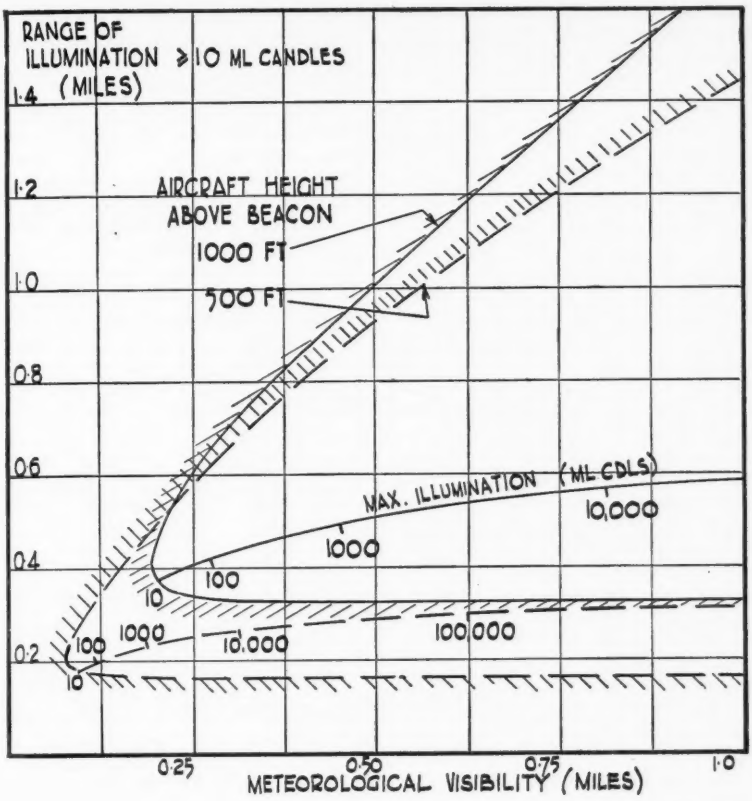


Fig. 1.

Visibility range of beacon to aircraft approaching over beacon as affected by meteorological visibility. Beam characteristics: Peak intensity, 50,000 c.p.; beam width to half intensity, 9°; mean elevation of beam, 15°.

the aircraft is likely to be when the pilot requires to identify the landing point. A properly controlled filament lamp can give intensities of the order of 10,000 to 100,000 candle power for a power input of about one kw. A light of this intensity has a range about double the prevailing meteorological visibility by night, with low background illuminations.

Suppose, for example, that the helicopter can be navigated to within ½ mile of the position of the beacon and approaches it at 700 ft. above the height of the beacon, then the limiting visibility in which the beacon can be seen will be about ¼ mile and for best effect the maximum intensity of the beacon should occur in a conical surface at about 15° to the horizontal. The pilot will not know in which direction to look for the beacon and it must be visible from the aircraft for some time to lessen the chance of its being missed; a very narrow beam, therefore, will not be suitable.

With such considerations as these in mind, a rotating lighthouse has been developed which provides four beams of about 9° in width to half intensity and with a peak intensity of 100,000 candle power. The beams are elevated at 11° to 19° to the horizontal, the precise angle being open to choice. Fig. 1 shows the range over which the light would be visible to an aircraft approaching over the beacon at 500 and 1,000 ft. above it in various meteorological visibilities. With zero background illumination and with full night vision an eye-illumination of 0.2 mile candles is sufficient,

but to allow for departures from these ideal conditions a limiting eye-illumination of 10 mile candles is assumed in drawing these curves. It will be seen, for example, that in the case of an aircraft approaching at 500 ft. when the meteorological visibility is ¼ mile, the beacon will become visible 0.6 miles away and the intensity will then rise to about 5,000 mile candles at 0.25 miles and fall again to the limiting value at 0.16 miles from the beacon.

If, as is probable, the aircraft is not approaching directly over the beacon, it will not be seen directly ahead. Assuming that the pilot makes no actual movement of his eyes in searching for the beacon, being largely occupied with his instrument panel, the image of the light will not reach the portion of the retina known as the fovea but that portion known as the parafovea. With low background intensity, parafoveal vision is the more sensitive up to 80° off the axis of the eye; in typical operating conditions, with extraneous lighting from the ground, the superiority of parafoveal vision diminishes[5]. It may be assumed conservatively that the effective angle of search for the beacon is limited to 30° on either side of the axis of the aircraft, so that the permissible lateral deviation of the aircraft from a track passing over the beacon is half the range of the light in the prevailing meteorological conditions. In this way the accuracy of navigation defines the limiting visibility for operation.

It is worth noting that the range of lights is not greatly affected by increasing their intensity by practical amounts, *e.g.* a tenfold increase in intensity will only result in a 15 to 20 per cent. increase in range.

Having identified the landing point, the pilot will require further guidance to land safely. From the work which has been done with fixed-wing aircraft and from experience obtained in night operation of helicopters, the visual information likely to be required if the pilot is to make a landing without recourse to instruments is:

(*a*) General wind direction.
(*b*) Descent gradient.
(*c*) Bank.
(*d*) Lateral displacement of the vertical plane containing the aircraft approach path from the touch-down point.
(*e*) Local wind direction at touch-down.
(*f*) Guidance as to height of aircraft above the ground during the touch-down.

This formidable list of requirements can probably be fairly simply provided for the helicopter. Bank and lateral displacement to the touch-down point could be given by a rudimentary version of the Calvert fixed-wing approach lighting system[6], in which a single cross-bar would serve as an horizon and the central line of lights could be shortened to the same length as the cross-bar, owing to the steeper approach path of the helicopter. The cross-bar and centre-line would constitute a reversed wind-tee to give the approximate wind direction. Descent gradient could be adjusted until the cross-bar lined up with a horizontal mark on the nose glazing of the helicopter. The effectiveness of this method would depend on the steadiness with which the approach path could be followed, since deceleration of the forward velocity of the helicopter would lead to an increase in the attitude of the body which might be corrected by reducing forward speed further; if it were ineffective, other means (*e.g.* a coloured sector light) would have to be adopted.

Local wind direction at touch-down can be given by a light pointer operated by a wind vane placed ahead of the aircraft and at the appropriate height, *i.e.* 5 to 10 ft. above the ground, or possibly by an assessment of drift relative to the pattern of lights on the ground. Guidance as to the height of the aircraft at touch-down can be shown either by a pattern of familiar dimensions (*e.g.* the individual lights of the lead-in and cross-bar being at constant distance apart), or by illumination of the ground to reveal texture.

The final touch-down would be made with the nose of the aircraft up to the (low) lights of the cross-bar which would always be placed in such a position that obstructions ahead would be well clear of the rotor. To avoid glare to the pilot, all the lighting might be indirect or, if direct lighting is used, the intensity should be variable by the ground controller, who would be in radio contact with the pilot. Indirect lighting is likely to be ineffective in conditions of ground mist or very low visibility so that it is unlikely to be universally applicable.

The foregoing equipment is envisaged in use in major passenger rotor stations where it can be assumed that a controller and fire-fighting staff will be required. An extremely simple device[7] which is described elsewhere, giving much of the required information, has been in use by B.E.A. Helicopter

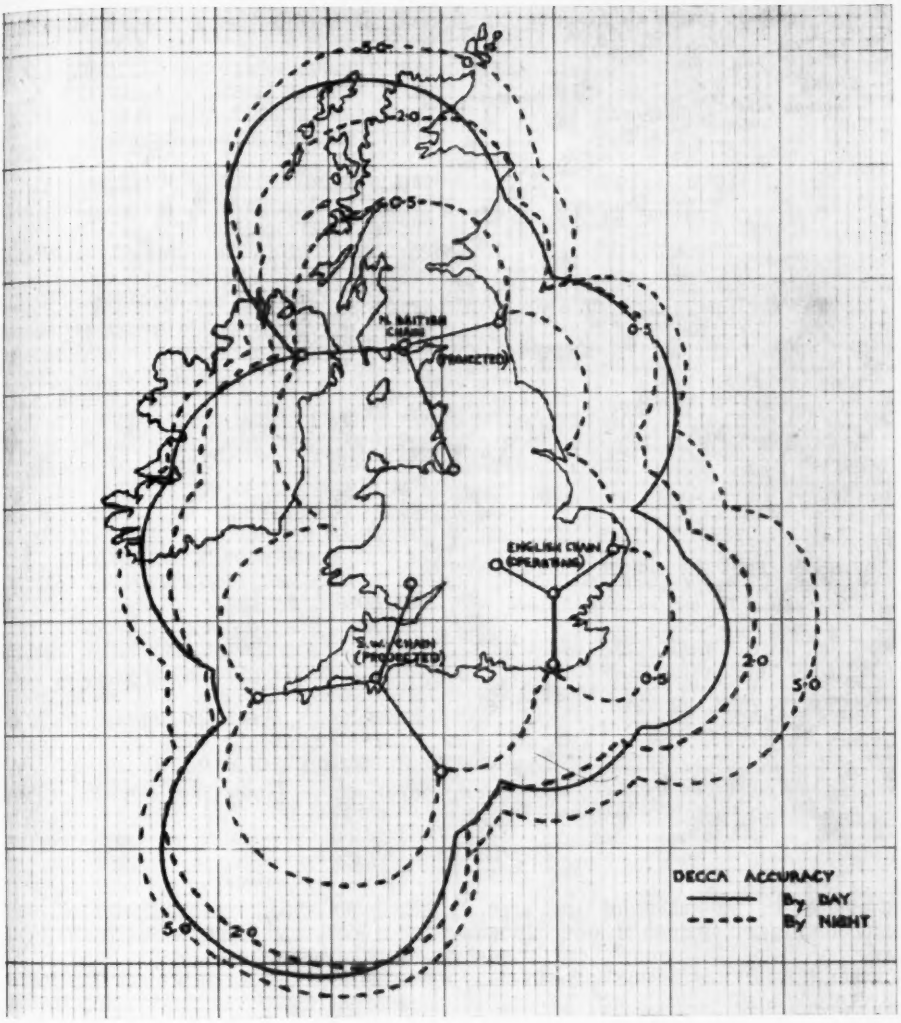
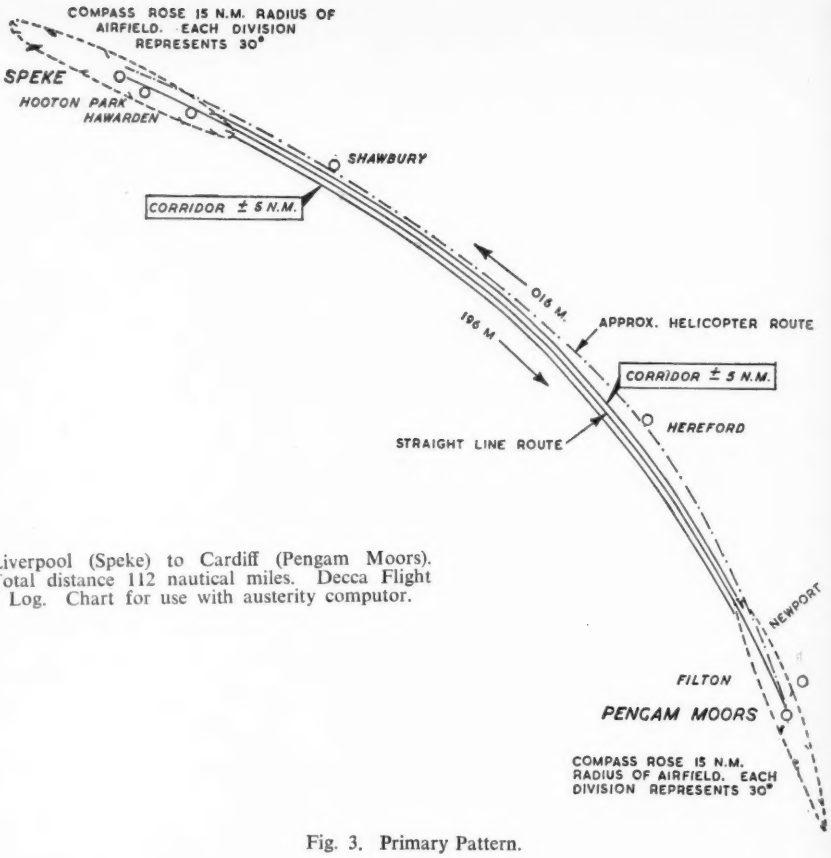


Fig. 2.
Fixing accuracy with the Decca Navigator System.

Experimental Unit and in certain applications could continue to be used in modified form. It is likely to constitute something of an obstruction at a restricted city centre rotor station, however, since it needs to be placed centrally in the landing area.

3. EN ROUTE NAVIGATION

A navigational aid is necessary to ensure that an aircraft reaches its intended destination in poor weather. A knowledge of position *en route* is also necessary for a number of reasons which, in some respects, are particularly applicable to the helicopter. A knowledge of position to a given order of accuracy, allows the helicopter to be flown at lower heights along predetermined routes without danger of collision with obstacles such as high ground, masts or other man-made obstructions. This would allow



Liverpool (Speke) to Cardiff (Pengam Moors). Total distance 112 nautical miles. Decca Flight Log. Chart for use with austerity computor.

Fig. 3. Primary Pattern.

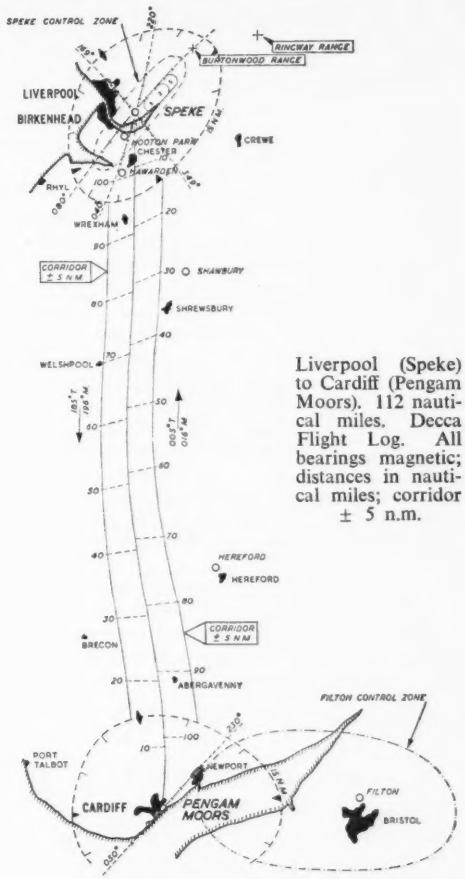
advantage to be taken of the general reduction in wind strength at lower altitudes when operating in strong head winds*. Further, if helicopters can be operated safely at relatively low heights, it may be possible to reserve to helicopters a relatively thin layer of air space (say, up to 2,000 ft. above the ground) in many areas, where they could be operated without ground control. Certain areas would have to be excluded from this freedom from control, *e.g.* the neighbourhood of airports, rotor stations and military airfields. To minimise the chance of air collision in de-restricted areas, scheduled helicopters could be operated to pre-arranged and co-ordinated flight plans, particularly in respect of height; the non-scheduled aircraft wishing to fly in the helicopter layer under instrument flight conditions would have to agree a flight plan with a central agency which was fully aware of all scheduled and other non-scheduled movements. Should actual control be considered necessary, a knowledge of the aircraft position to the controller is essential.

On economic grounds an existing area coverage system of navigation is most suitable; it must also be capable of being used at low altitude. The Decca Navigator System[8] meets these requirements since it is a medium frequency system which is already widely in use for marine and air purposes in the United Kingdom and elsewhere. Fig. 2

* Flight near the ground in regions of high wind shear is normally unpleasant in a fixed-wing aircraft, but the experience so far accumulated in helicopters suggests that the occupants are not affected to nearly the same degree.

shows the fixing accuracy offered by this system over the U.K.* It will be seen that most of the U.K. with the exception of the north of Scotland is covered by night to an accuracy of at least ½ mile on 95 per cent. of occasions; during the daytime the accuracy is improved.

In its simplest form the method of operation is to compare the readings from two of three instruments (known as Deccometers) with a map overprinted with hyperbolic co-ordinates, thereby fixing the aircraft position. On a fixed-wing aircraft this generally requires the assistance of a navigator, and on contemporary helicopters, which demand the pilot's attention almost all the time, the procedure is impracticable, and a simplified form of presentation is clearly essential for single-crew operation. The Decca Navigator Company have under development an ingenious device known as the Flight Log[9]. For this device a conventional map is transformed in such a way that two of the sets of hyperbolic Decca co-ordinates become orthogonal straight lines. The movement of a cursor relative to the map is controlled in two directions at right angles by the same signals which actuate the corresponding Deccometers. The distortion introduced into the conventional map by this device depends on the position of the area considered in relation to the Decca Chain in use. At points some distance from the Chain the distortion can be considerable, as Fig. 3 shows; circles drawn around the two airports have become elipses of high eccentricity, while the straight line joining them has curved through 40° to 50°. To minimise this deficiency, instead of controlling each degree of freedom of the cursor by a signal which actuates a single Deccometer, each degree of freedom is controlled by a combination of signals so chosen as to reduce distortion. Fig. 4 shows that the same route which was badly distorted in the previous arrangement, by making use of these so-called secondary patterns, now bears a fairly close resemblance to the conventional map†. Additionally, different scales can be given to the two degrees of freedom of the Flight Log and this can also be applied to increasing the overall scale of the chart at terminal points.



Liverpool (Speke) to Cardiff (Pengam Moors). 112 nautical miles. Decca Flight Log. All bearings magnetic; distances in nautical miles; corridor ± 5 n.m.

Fig. 4. Secondary Pattern.

Trials of this equipment have already shown it to be an effective form of navigational aid and, provided that it can be shown to function reliably, it will be of particular value to helicopters in flight along predetermined tracks and also in the case of diversion. The signals received from the system are liable to interruption by "static" or other causes and it may then give a false indication. This is immediately apparent by a discontinuity in the pencil trace of the aircraft flight pattern which is marked on the chart. In these circumstances, it is essential that the chart can be reset correctly. A marked improvement in the helicopter's handling characteristics in cruising flight is necessary before the pilot can be expected to do this unaided in a reasonably short time,

* Only the London Chain is at present in operation, but the S.W. and N.W. Chains are expected to be in service in 1951.

† It should be emphasised that the inherent accuracy of the system is not increased by use of secondary patterns.

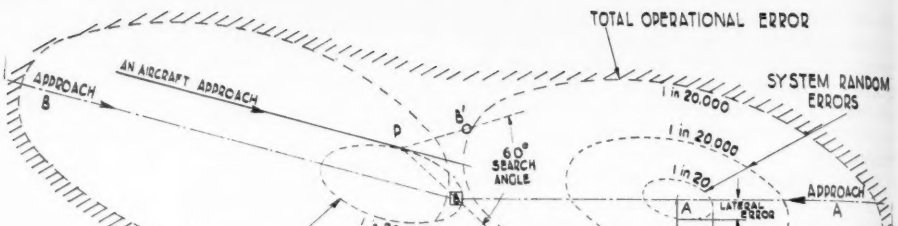


Fig. 5.
Sketch illustrating use of the Decca system as an approach aid.

i.e. he must be able to give most of his attention to the operation for perhaps half a minute without the helicopter getting out of control.

The helicopter will necessarily be carrying V.H.F. communications (duplicated to guard against breakdown) and in the event of the breakdown of the navigational aid under instrument flight conditions, it could be directed to a landing field (airport or rotor station) by V.H.F. direction finding.

4. APPROACH PROCEDURE IN POOR WEATHER

Now let us consider the problems involved in arriving safely at the destination. Often this will lie near the centre of large towns and there may be high obstructions in the neighbourhood. There may also be other aircraft in the vicinity, landing or taking off from the same point. Suppose that no other aid than the navigational aid already described is available; what limitations would this impose on operations?

Firstly, there will be inaccuracies in the aircraft position as calculated due to local topography. This systematic error can be eliminated by noting the mean Decca co-ordinates of the landing point over a sufficiently long period. Then there will be a random error due to variations in the medium through which signals have passed from the transmitting stations. Referring to Fig. 5, assume that an aircraft is flown to the mean co-ordinates of a point *A* which is shown on the right-hand side of the figure; a probability can be attached to the aircraft actually being within ellipses centred around *A*. Ellipses showing a probability of one in 20 and one in 20,000 are illustrated for a representative point in the United Kingdom at night when these errors are largest.

Over and above the system random error there will be operational inaccuracies introduced by the pilot in observation of the display and in acting on the information this gives; for *purposes of illustration*, it is assumed that these operational errors double the basic system random error. Thus, if the pilot intends to place the aircraft at point *A*, on one occasion in 20,000 it may lie outside the ellipse on the right-hand side of the figure which is marked "total operational error." To make remote the chance of colliding with obstacles, the limiting cloud base would then have to be set at some height above the highest obstruction within this ellipse.

It will be appreciated that since the incidence of limiting cloud height will be small, the actual proportion of approaches on which the aircraft would find itself outside the one in 20,000 ellipse of total operational error in non-visual conditions will be considerably less than one in 20,000.

Consider now a typical approach procedure; this is illustrated on the left-hand side of the figure. The pilot intends to break cloud, if it is at the limiting height for the station at which he is landing, at a point corresponding to *A*; he knows that he is not in danger of collision with ground obstructions. In fact, his track may not pass through the rotor station and its beacon *B*, but lie to

one side as illustrated; suppose his track corresponds to a one in 20 error wholly in the lateral sense. The aircraft continues on track beneath cloud until the rotor station is identified by means of its beacon *B* and thereafter lands visually in the manner described. What will decide the limiting visibility conditions for the station? If it is assumed that for operational convenience it is desirable that the rotor station beacon be identified on the first run up to the station 19 times out of 20, the limiting case will correspond to recognition at point *P* when the rotor station beacon will just lie on the edge of his angle of search (assumed before to be 30° on either side of the aircraft track) and when the visibility is such that the light is just visible, *i.e.* when the range of the light is double that of the lateral error of the aircraft's position. As a rough rule the range of a beacon of the type envisaged is about double that of the meteorological visibility, so that the lateral error of position to the degree of probability considered acceptable will be of the same order as the limiting meteorological visibility.

Occasionally, the aircraft will pass out of range of the rotor station beacon *B*. This will not put it in any danger since it will be flying in an area in which obstructions are marked and below the cloud base and it can continue to point *A*, turn and repeat the search for the beacon. The system errors can persist for many minutes at night, however, and much time might elapse before the station was identified. If satellite beacons *B'* and *B''*, flashing distinctive signals, are placed on each side of the rotor station as shown in the figure, the chance of overshooting the station would be very much reduced.

It is in the foregoing manner therefore that the accuracy of the navigational aid sets a limit to the cloud ceiling and visibility in which it is safe and convenient to operate into any station. It will be clear from the arguments that in setting the visibility limits it is the lateral accuracy which is important and there are various ways in which the best possible results can be obtained. At points far from the Decca Chain the ellipse of error becomes more and more eccentric; if the approach path is arranged to lie along, or nearly along, the major axis of the ellipse (as illustrated in Fig. 5), the visibility limit can clearly be lower than if the approach path lies along the minor axis. At points nearer still to the fringe of the cover, the best results will be obtained by "homing," *i.e.* in arranging the approach lane to lie along a Decca co-ordinate which passes through the landing point. The procedure might then be to make use of the Flight Log to navigate the aircraft to within a few miles of the landing point, pick up the appropriate Decca co-ordinate and fly down this, making use of the Deccometer as a "left-right" indicator, until the landing point is identified. The Flight Log will greatly simplify "homing" by giving the pilot his position, albeit to a lower order of accuracy, in a pictorial form.

Another method of increasing accuracy which has been suggested is to eliminate the system random error by monitoring from a known point on the ground. The operational objection which might make this process unacceptable arises from the fact that quite rapid changes in the random error can occur and, unless the aircraft receiver can be automatically monitored, the task of making use of the information received from the ground might well be beyond the unaided ability of a single pilot. The expense of ground-based equipment, probably needing additional staff for its operation, is an added reason why this scheme appears unattractive.

So far, the problems associated with landing a single aircraft in poor weather conditions have been considered. As time goes on the movement rate at some rotor stations will increase to the level at which some form of traffic control must be instituted to avoid air collisions. The author does not believe that it will be practical to maintain schedules so closely that control in the neighbourhood of rotor stations can be relaxed. Inevitably departure delays for various causes will persist and the helicopter will continue to be more affected by winds than fixed-wing aircraft. No operator will want to employ such slow schedules that he has a margin to meet most conditions of departure delay or time lost *en route* due to head winds, and further expend unnecessary flying hours to avoid arriving early when the worst does not happen.

It is probable, therefore, that in regions of high traffic density, *e.g.* near rotor stations, a positive form of traffic control must be instituted. The remarkable manoeuvrability of the helicopter and its slow flying characteristics undoubtedly make this an easier task than for the fixed-wing aircraft. By this it is not meant that, if called upon by

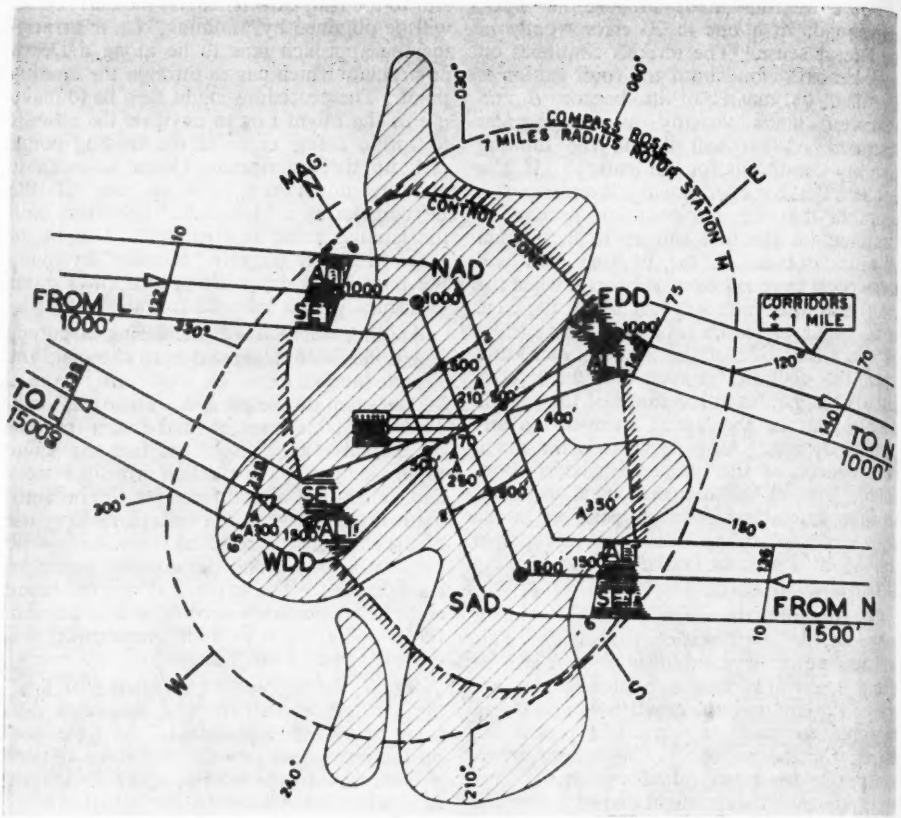


Fig. 6.
Flight log chart for rotor station *M*.

control to hold position, the helicopter will hover, but it can readily maintain position within $\frac{1}{4}$ mile of any holding point by circling at or near its minimum power speed. A pictorial presentation of position such as that offered by the Flight Log greatly simplifies the carrying out of such manoeuvres.

Figure 6 illustrates the principles on which control might be maintained at a rotor station. It is presented in the form of a Flight Log Chart at a point where distortion and accuracy are appreciably worse than average. The basic principle is to segregate incoming and outgoing traffic. A helicopter travelling between *L* and *M* would contact control at *M* some minutes before entering the rotor control zone, for purposes of giving prior warning and of obtaining the atmospheric pressure at the station. Some 6 miles short of the station and two to three miles short of the northern approach datum (shown as *NAD* on the chart), the altimeter is adjusted and the aircraft continues flying at its nominated cruising height to *NAD* and then down the approach lane, losing height meanwhile, to be at the safety height for the station about two miles short; by then, it should have broken cloud, and the approach and landing procedure would be as described before. If other aircraft are landing, the controller can instruct the pilot of the aircraft which is being considered here to hold his position at 6 miles, 4 (the approach datum) and two miles from the rotor station. When the helicopter leaves *M* for *N*, it goes out by the eastern departure datum (shown as *EDD*) along a path at right angles to the approach lanes. It would have climbed to its

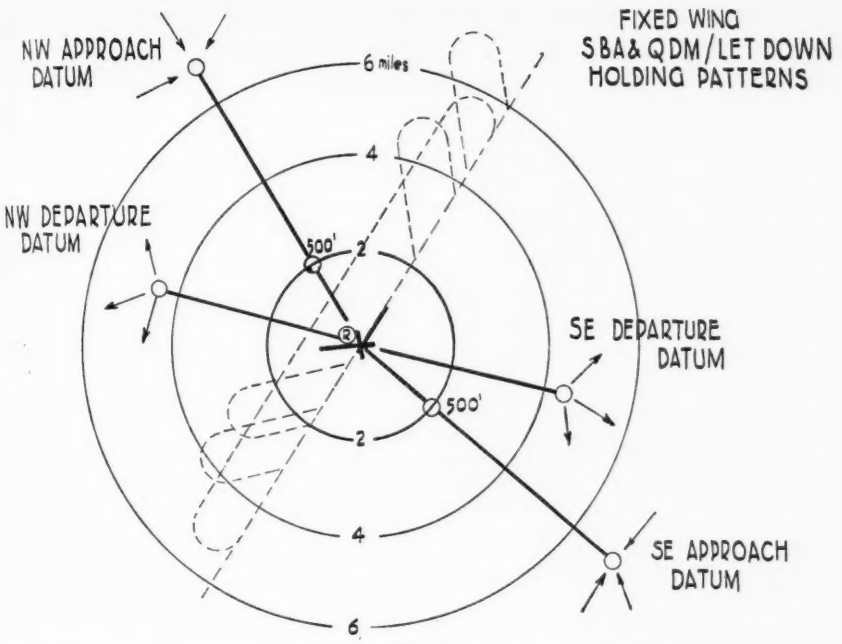


Fig. 7.
Sketch illustrating procedures adopted during trials of controlled approach to fixed-wing airport.

nominated cruising height by the time *EDD* is reached and before leaving this point, on course for *N*, altimeters would have been adjusted to the area setting.

With an arrangement such as this and with never more than one aircraft in an approach lane and out of sight of control, about 12 to 15 landings an hour might be made. Since the departure route is directed at right angles to the approach route, take-off could safely be undertaken when aircraft are in the far ends of the approach lanes so that an overall movement rate of 20 to 25 helicopters an hour should be possible, and this figure is likely to meet requirements at most rotor stations for some time to come.

Consider now the case of the helicopter which wishes to enter a fixed-wing airport. In view of their differing flying characteristics, in particular their ranges of possible speeds and manoeuvrabilities, it is advisable to keep the two classes of aircraft as separate as possible. Fig. 7 is a sketch map of the procedure adopted in some trials conducted with a helicopter at a fixed-wing aerodrome. Helicopter approach and departure lanes were chosen so as not to lie along any of the runways and lying at the greatest angle to the single instrument runway. Approach datum points were agreed and the helicopter could be held there by control, or at the edge of the bad weather circuit of the airfield about two miles from the centre. The helicopter landing point was near to and in sight of the control tower. A helicopter approaching from the other side of the runway to that on which the control tower was situated could be held at the edge of the airfield by control until the runway was clear and could be crossed. In a period when there were about ten fixed-wing movements an hour, twelve helicopter movements were made satisfactorily. One real helicopter took part and others were simulated by a controller elsewhere in the control tower.

Undoubtedly, the control problem at a fixed-wing airport with heavy traffic will be more serious than at rotor stations and methods other than those outlined above may have to be adopted, or the helicopter may even have to be kept outside the airport proper.

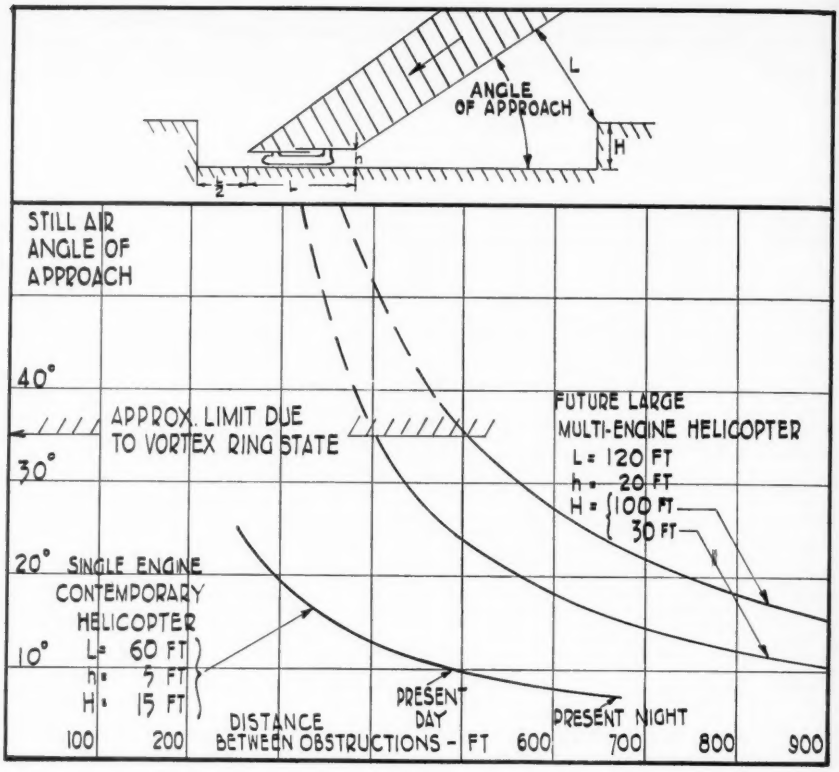


Fig. 8.
Linear dimensions of rotor station as affected by approach angle.

To return to the approach to a rotor station in poor weather; if the assessment previously given is correct and reliance is placed entirely on the navigational aid for approach purposes, the regularity achieved is likely to be badly affected, except at favoured points. An area system of navigation is fundamentally unsuited for approach in severe weather conditions; in these circumstances, a device is needed which increases in accuracy as the obstructions near the landing point are approached. This implies the use of a radio aid situated on or near the landing point. Many difficulties are likely to be encountered during the development of such an aid and it is all the more important that work on it should be progressing. Such an aid, moreover, could serve as a short-range standby navigational aid in the event of failure of the primary navigational aid.

5. OPERATION FROM RESTRICTED SITES

For helicopters to have a future as public transport vehicles, they must be able to operate from built-up areas; operating between airports, they are merely acting as a complicated alternative to fixed-wing aircraft. Vacant land in built-up areas is difficult to secure and furthermore, it is expensive. Eventually, helicopters may operate from the roofs of buildings and so not deny the use of ground to other interests, but in this case there are limits to the dimensions of building which can be put to other use. In either case, the smaller the landing ground which can be used the better.

Consider first the limitations imposed on site size by landing. It is fairly obvious that the steeper the approach the less space that

is needed. Fig. 8 suggests a relation between approach angle and site length. The clearances of half an aircraft length ahead and no approach nearer than an aircraft length to an obstacle behind the touch-down point are arbitrary; in practice, the approach path may not be a straight line but curved so that the latter part is substantially vertical, but this will tend to make clearances greater when operating into a site of given size.

Present-day single-engined helicopters normally approach at 7° to 10° to the horizontal relative to the air (the lower figure corresponding to night operation); on the basis of Fig. 8, a site length of 500 to 750 ft. surrounded by 15 ft. obstacles would be needed. In practice, smaller sites than this have been used but that is merely a reflection of the possible conservatism of the assumptions. Such a dimension is large and not likely to be found easily in towns; to accommodate a bigger helicopter of the future a much larger space would be needed, particularly amid high buildings.

Figure 8 shows that as the angle of approach increases the site length gets smaller and gets down to quite manageable dimensions of 400 to 500 ft. between buildings by the time it has reached 35°. The reason for choosing 35° as a planning limit is that in the absence of wind and at the rates of descent visualised, the helicopter rotor is liable to be operating in the vortex ring condition at angles of descent relative to the air in excess of 45°. In its milder form, this results in roughness and, in its extreme form, may lead to temporary loss of control (*e.g. see* Ref. 10, 11).

Figure 9 shows the site areas required. Since approaches are normally made into wind, these are based on the square of the length derived in the previous figure—an assumption which is rather pessimistic at the larger sizes. The figure suggests that the area needed by present-day single-engined helicopters not operating in a built-up area is about six to 10 acres. A town site for a large multi-engined aircraft approaching at 35° would have to be of about four acres in size. A site much smaller would not give sufficient room for ground installations and parking of other aircraft.

Why do present-day helicopters approach at such shallow angles and why can it be hoped that future multi-engined aircraft will approach at a much steeper angle? As a result of flight tests, regimes in which the single-engined helicopter will be unsafe in the event of engine failure have been established. If operating far inside the left-

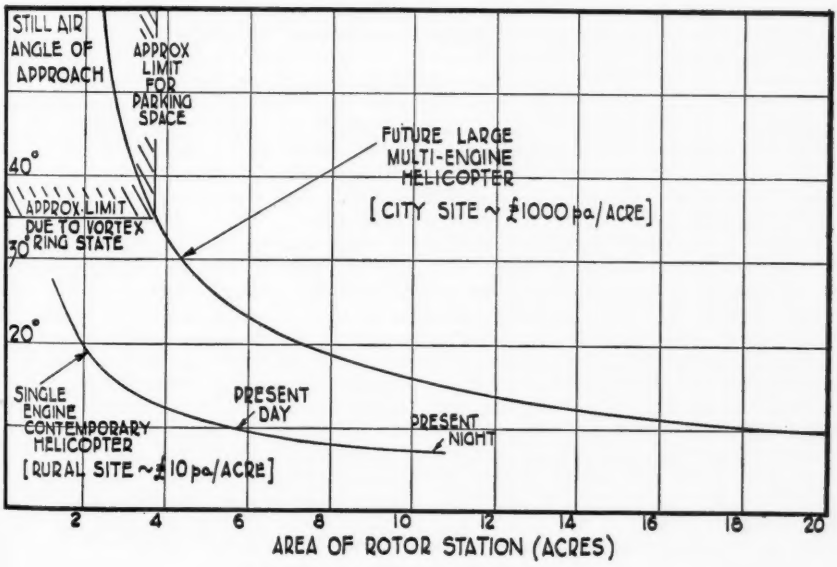


Fig. 9.
Area and cost of rotor station as affected by approach angle.

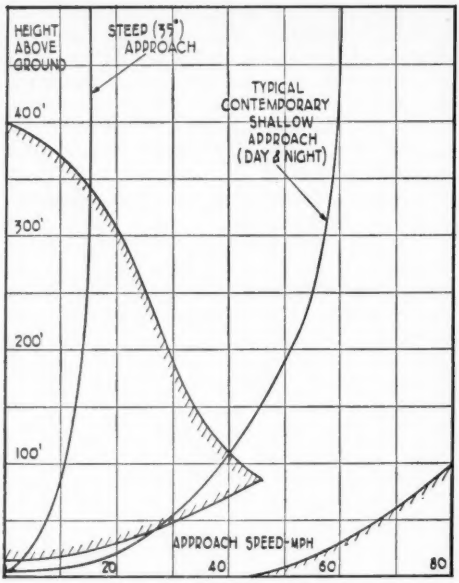


Fig. 10.
Relation of approach procedures to danger zones in zero wind.

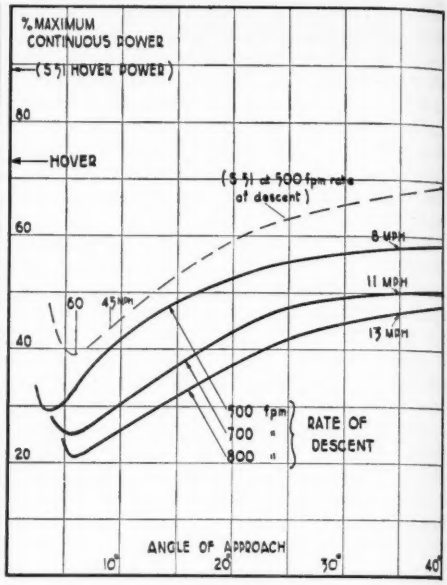


Fig. 11.
Effect of angle of approach on power required for steady flight in zero wind.

hand boundary shown in Fig. 10, even with a very skilled pilot, after an engine failure the velocity of impact with the ground may be excessive. The present-day technique for landing single-engined helicopters varies to some extent from pilot to pilot, but by and large it involves an approach at about 45 to 60 m.p.h. (*i.e.* near the aircraft's minimum power speed) until the ground is approached and the downward and forward speed is checked. It will be seen that this type of approach lies near the boundary of the danger zone for a short while only; in the event of engine failure, the pilot might land short of the point he was making for but the aircraft would be under control and excessive rate of descent would not develop. On the other hand, if a steep (35°) approach with the rate of descent limited to current values is considered, the forward velocity must be kept low and the aircraft lies within the danger zone for the last 300 to 400 ft.

Figure 11 shows the power required as affected by angle of approach. The Sikorsky S.51 in a steep approach at 500 ft. a minute rate of descent requires two-thirds of its available power. The more normal approach technique corresponds to using about 40 per cent. of available power. Also shown in the figure is the power required for a project design of a much larger helicopter employing two engines; in steep descent this is about 50 per cent. of the continuous power available from the engines. If an engine fails, the pilot could open up the remaining engine and substantially continue his original approach path; whether this presents problems from the handling standpoint is a matter which awaits evaluation on a twin-engined helicopter.

Pilots also report that there is sluggishness in response to controls at low air speeds. Since it has been shown that the lag between stick movement and rotor movement is small, the impression of sluggishness must arise from the fact that velocities or displacements do not immediately follow a movement of the thrust vector due to the inertia of the body. In this respect, a multi-rotor helicopter should be better, provided that control is obtained primarily by varying the relative magnitudes of the rotor thrusts rather than altering their inclinations. Other factors which may worsen the impression of sluggishness are the heavy control forces of some types and the hypersensitivity of the body to air flow from

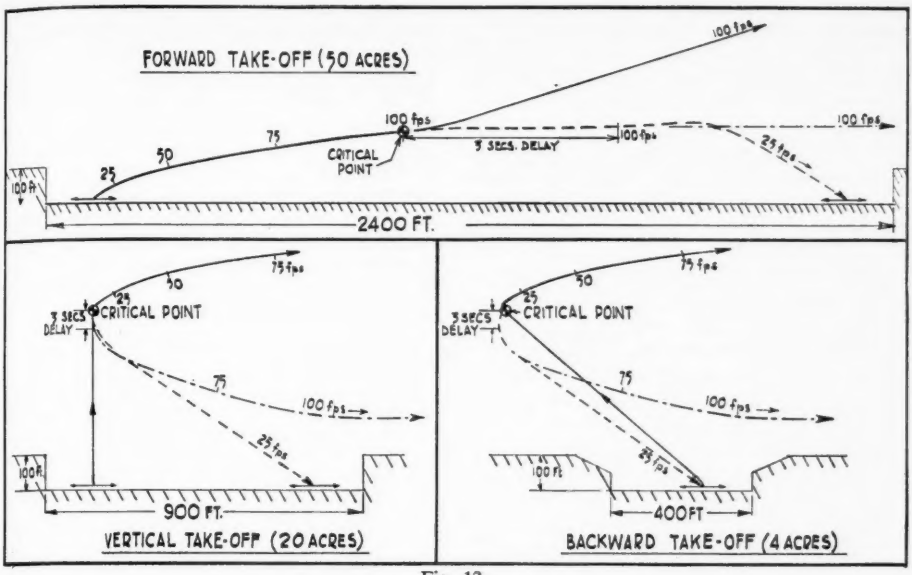


Fig. 12.
Safe take-off procedures and effect on site dimensions.

above, which will be present in slow, steep descents. The use of light controls, careful aerodynamic design of the body and the more uniform air flow over the body from a multi-rotor aircraft should all help to improve matters.

Consider now how take-off will affect the size of sites suitable for safe operation. If a forward take-off is envisaged, the flight path may be as shown at the top of Fig. 12. The aircraft is accelerated and climbed to a speed and height such that it can maintain height on one engine and be clear of surrounding obstructions. If an engine fails at this point, the pilot has the choice of continuing to climb or of returning to the airfield; a period of delay while he is making up his mind must be allowed. A rather large dimension of nearly half a mile results and, if this is made up of two "runways" 150 yards wide to allow for emergency landings into wind, about 50 acres are required.

Suppose a vertical ascent is made to a critical point such that if an engine failed the helicopter could either return to the landing field or pick up a sufficient forward speed (losing height in the process) to maintain height on the remaining engine without danger of collision with ground obstructions. This procedure is illustrated in the left-hand lower diagram of Fig. 12. The pilot will not wish to descend vertically, since he will be in danger of loss of control from the rotor operating in the vortex-ring state and furthermore, it will not be easy for him to see immediately below, therefore he will be driven to make his approach back on to the landing ground at a steep, but far from vertical, angle. The area of site required for this technique would be about 20 acres.

Suppose now the process is continued a stage farther; it is assumed that the pilot takes off backwards and at about 45° to the horizontal, keeping his take-off point in view and the aircraft nose into wind. If the engine fails at the critical point, he has sufficient height to fly away on one engine or to descend on to his take-off point, and this phase of the take-off becomes identical with that of the normal approach. The area required, therefore, is the same as that already suggested as meeting landing requirements, *i.e.* about four acres. The ability to maintain control of the aircraft when the engine fails and during the period while adjustments are being made to the throttle of the remaining engine and the collective pitch, will require to be demonstrated. Until such matters as this have received practical trial the requirements of space for rotor stations

cannot be specified with a sufficient degree of confidence.

The broad requirements which a multi-engined helicopter should meet for operation over and into built-up areas are:

(*a*) The helicopter should be able to maintain height after engine failure with the remaining engine(s) operating within their continuous rating by a margin of power to allow for manoeuvres, rough air and deterioration.

(*b*) The helicopter should be able to follow a normal approach path after engine failure with the remaining engine(s) operating within the take-off rating.

(*c*) The helicopter should be able to make a safe touch-down on a restricted area in the hands of a competent pilot after engine failure.

(*d*) The helicopter should be able to execute a take-off manoeuvre such that, after engine failure at any stage, it can return safely to a restricted take-off point or fly to a less restricted landing area.

6. CONCLUDING REMARKS

A good deal of ground has been covered and it has not been possible to deal as fully with some aspects as, perhaps, they warranted; no description of experiments or operational experience which formed the basis of much of the discussion has been included. Certain important aspects, such as cruising speed requirements in relation to winds and scheduling, weather forecasting, emergency landing fields and the general organisation of operations, have not been touched on at all.

Enough has been said to show that there are many problems of a different character from those experienced by fixed-wing aircraft which require attention. Given the co-operation of research establishments, designers and operators, they can be overcome, and moreover, profiting by the accumulated experience of years of aviation, in a much shorter space of time than was taken to solve the corresponding problems for fixed-wing aircraft.

The author wishes to acknowledge the assistance given in preparing figures by Mr. R. H. Chowns and the Decca Navigator Company. While the views expressed are personal, he would also acknowledge the stimulus received from working with those engaged in helicopter operation in British European Airways, even though they may disagree with some of the conclusions reached. The author is also indebted to the British European Airways Corporation for permission to give this paper.

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MEASUREMENT OF DOWNWASH AT A MACH NUMBER OF 1.45 BEHIND TWO WINGS OF FINITE SPAN

by

W. F. HILTON, Ph.D., A.F.R.Ae.S.

SUMMARY

Measurements were made of the downwash effects behind two finite wings 3.1 per cent. thick, having square and 20° raked tips respectively. The tests were conducted at a Mach number of 1.45 and a Reynolds number of 1.2 millions by traversing a yawmeter 1.62 chords behind the trailing edge of the finite wings.

In general, a maximum downwash of the order of $\frac{1}{2}°$ per degree of wing incidence was observed in that portion of the tip Mach cone behind the wing, and a maximum upwash of similar magnitude was observed in that part of the tip Mach cone situated outboard of the wing.

Thus it is apparent that these effects are large enough to affect the lift on any surface situated in the tip Mach cone behind a finite wing. In particular, placing the rear surface in the downwash region behind a finite wing, will tend to reduce the overall lift while placing it in the upwash region will tend to magnifiy the variations of lift initiated by the finite wing.

INTRODUCTION

Several phenomena had led to the supposition that downwash effects from the tip of a finite supersonic wing were of a fairly large magnitude, and it was considered useful to make a brief examination of the subject.

The 12-inch tunnel at the National Physical Laboratory in which these tests were made is shown in Fig. 1. It suffers from the disadvantage of an irregular distribution of Mach number, as shown in Fig. 2, and is rather worse than the average supersonic tunnel in this respect. The tunnel also showed a variation of wind direction of the order of 1° over the area tested. It was felt that a quick answer, correct in sign and to 5 per cent magnitude, would advance this

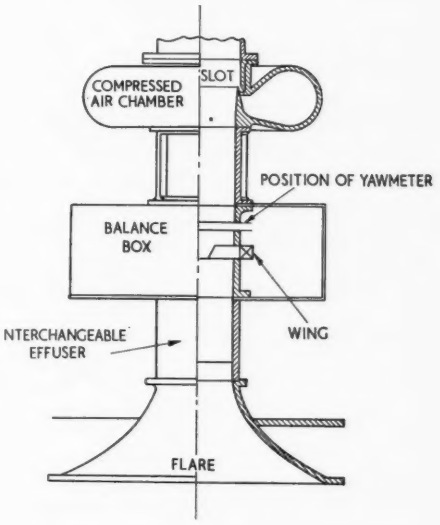

Fig. 1.
Cross section of supersonic wind tunnel.

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little-known subject appreciably*. The consistency of the observations, in fact, is rather better than this, as will be seen from Fig. 4. No attempt could be made to check Lighthill's theory[1] that the two-dimensional supersonic downwash or upwash effect is of the third order of small quantities, since flow inclination could only be measured inside the tip Mach cone, where the flow was three-dimensional.

NOTATION

c wing chord
C_L lift coefficient
K circulation of vortex
L lift per unit span of wing
r radius of vortex
t vortex "age" (sec.)
V velocity of air flow
w circumferential velocity of vortex
α wing incidence
ε angle of upwash (downwash is negative)
ν kinematic viscosity (0.000152 ft.²/sec. in the experiments quoted)
ρ air density

*This was done in April 1947 and first reported in June 1947.

DESCRIPTION OF APPARATUS

The yawmeter consisted of a short length of EC 1250 aerofoil section*, with two

*Aerofoil with maximum thickness 12 per cent. of chord, situated 50 per cent. of chord back from leading edge. Nose shape elliptical, with cubic tail.

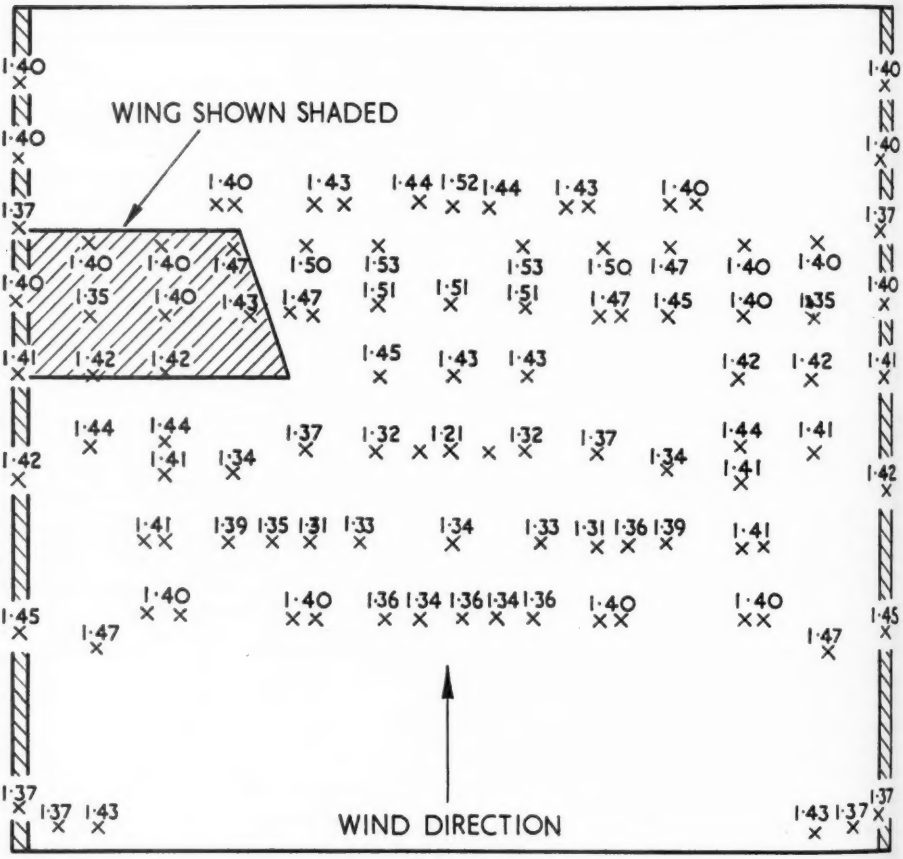


Fig. 2.
Distribution of Mach number in vertical plane.

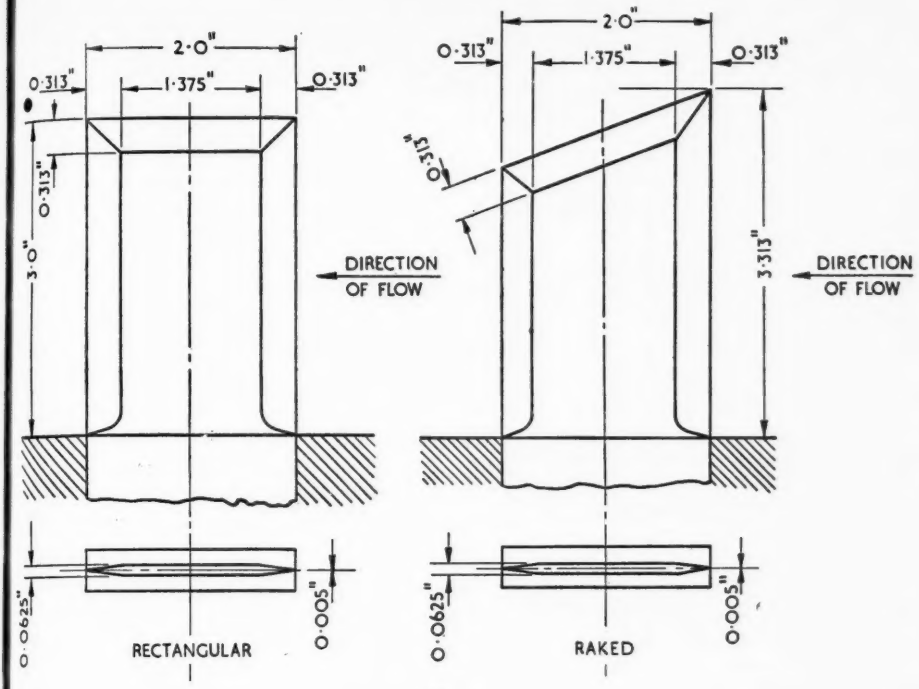


Fig. 3.
Sketch of wings tested.

pressure holes symmetrically disposed, one on each side of the aerofoil as shown in Fig. 4. The yawmeter was designed for subsonic use, the holes being situated 15 per cent. of the chord aft of the leading edge, where the pressure was found to be most sensitive to change of angle of incidence. The reading is observed as the pressure difference between the two holes. Although this yawmeter was not perhaps ideal in design for supersonic speeds, it had an approximately linear calibration curve of ample slope, say one inch of mercury per degree of yaw, as shown in Fig. 4. Thus one-tenth of a degree of yaw could be observed with certainty, and hundredths of a degree could be estimated.

The traverse gear was such that the yawmeter bar was held parallel with its leading edge 3.02 in. and its pressure holes 3.24 in. (1.62 c) behind the trailing edge of the wing. Modification would have been necessary to test at any other distance back. Thus only upwash and downwash could be observed, and spanwise "cross-wash" could not be measured. The wing thickness was 1/16 in., the chord was 2 in., giving a thickness ratio of 3.1 per cent. The wings had the plan forms shown in Fig. 3, and were rotated about their mid-chord in order to change the angle of incidence.

No attempt was made to determine absolute angles of yaw, but in each case, a yawmeter traverse of the empty tunnel was made and used as a datum. To make a determination of absolute magnitude it would have been necessary to measure any twist present in the streamline bar supporting the yawmeter, and to calibrate the yawmeter at the same point in the tunnel but supported from the opposite wall. It was felt that this would be a waste of effort, and that the empty tunnel datum would be sufficiently accurate in view of the poor distribution of Mach number and wind direction.

The experimental procedure adopted was to set the yawmeter to traverse parallel to

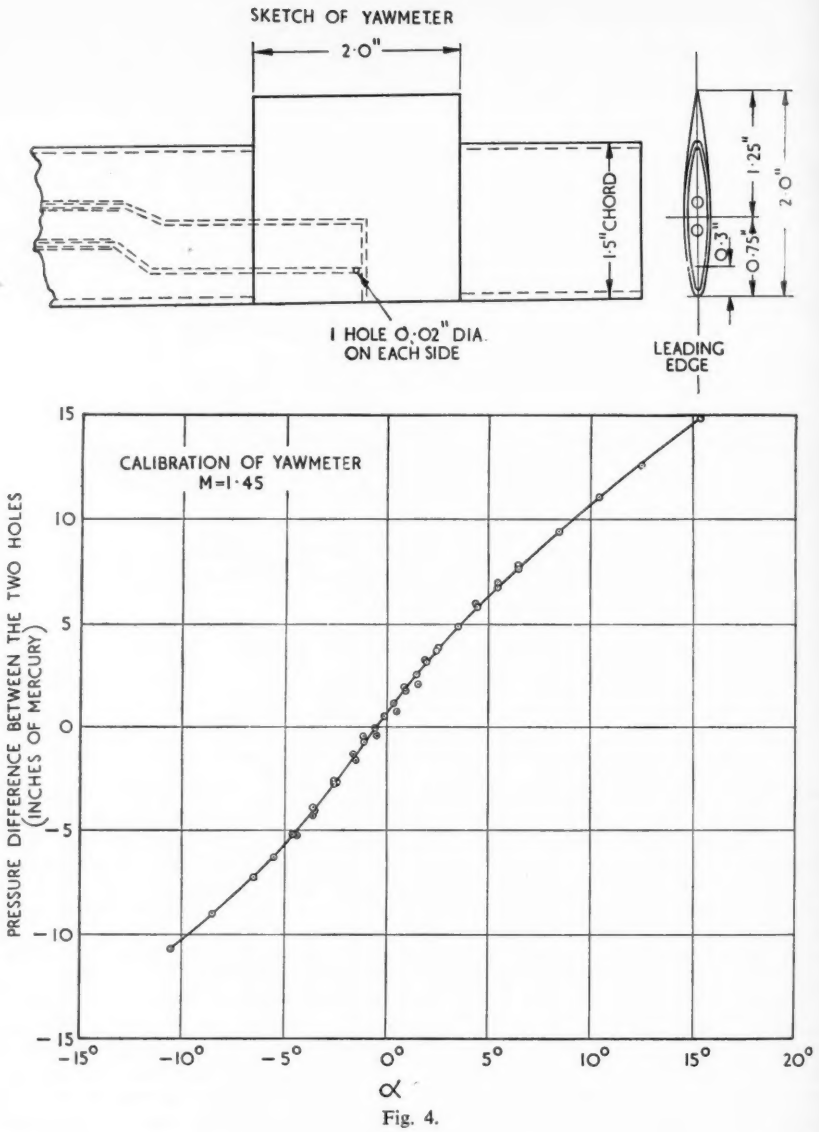
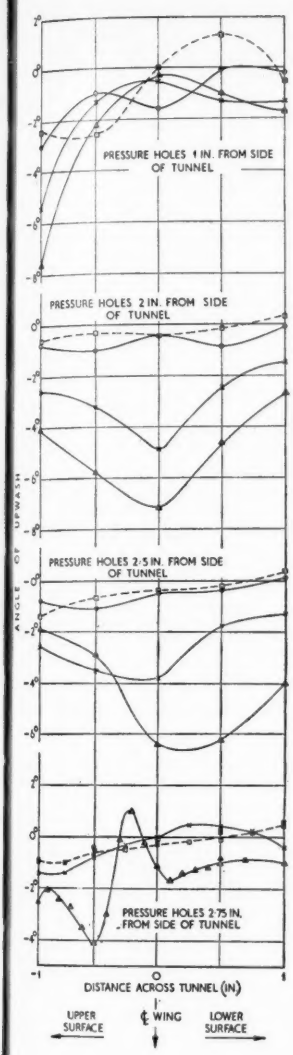
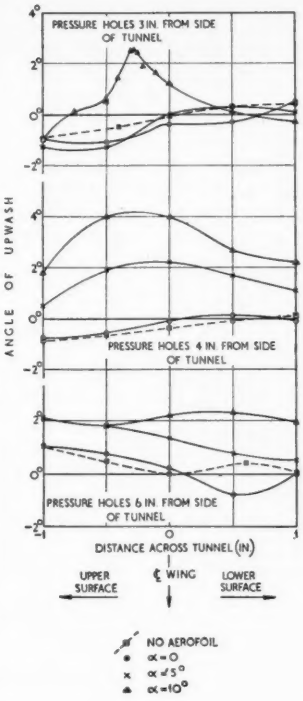


Fig. 4.

the direction of wing thickness at some particular spanwise station behind the wing tip. Traverses were then made at 0°, 5° and 10° incidence of the wing, and then the wing was removed to take an empty tunnel calibration, without disturbing the yawmeter. This procedure was repeated for other spanwise positions. The distance of traverse of the yawmeter was 2 in., parallel with the direction of wing thickness. The geometrical configuration was such that all measurements made were within the Mach cone from the tip.

The tunnel was run in a reasonably dry condition, by re-circulating the dry air from the 25 atmosphere air compressors until


PRESSURE HOLES 1 IN. FROM SIDE OF TUNNEL
PRESSURE HOLES 2 IN. FROM SIDE OF TUNNEL
PRESSURE HOLES 2·5 IN. FROM SIDE OF TUNNEL
PRESSURE HOLES 2·75 IN. FROM SIDE OF TUNNEL
ANGLE OF UPWASH
DISTANCE ACROSS TUNNEL (IN)
UPPER SURFACE
℄ WING
LOWER SURFACE



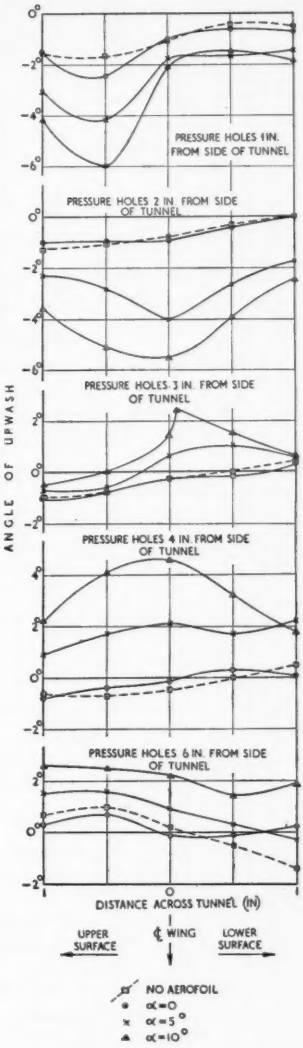

PRESSURE HOLES 1 IN. FROM SIDE OF TUNNEL
PRESSURE HOLES 2 IN. FROM SIDE OF TUNNEL
PRESSURE HOLES 3 IN. FROM SIDE OF TUNNEL
PRESSURE HOLES 4 IN. FROM SIDE OF TUNNEL
PRESSURE HOLES 6 IN. FROM SIDE OF TUNNEL
ANGLE OF UPWASH
DISTANCE ACROSS TUNNEL (IN)
UPPER SURFACE
℄ WING
LOWER SURFACE
NO AEROFOIL
α=0
α=5°
α=10°

Fig. 5.
Yaw behind raked wing.

Fig. 6.
Yaw behind rectangular wing.

there was no visible condensation. The excess air due to continual injection of air was allowed to escape through a louvre.

Measurements of total lift of these finite wings were also made on the electric balance of the 12 in. tunnel. This balance is described in Reference 2. The forces have been reduced to coefficients based on the actual wing area in each case, and are shown plotted in Fig. 9. The Mach number of the tests was 1.45 and the Reynolds number 1.2 millions.

DISCUSSION OF RESULTS

The chief result established by these experiments is the existence of a downwash behind a supersonic wing of approximate magnitude $\frac{1}{2}°$ per degree of wing incidence; an upwash of similar magnitude exists outboard of the wing tip, together with a

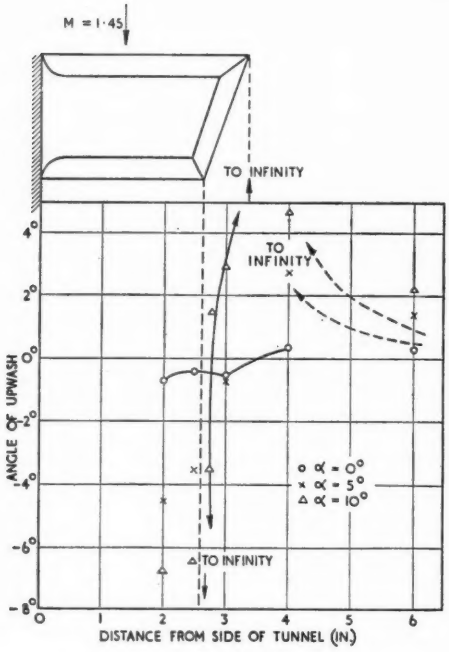


Fig. 7.
Spanwise distribution of maximum downwash behind raked wing.

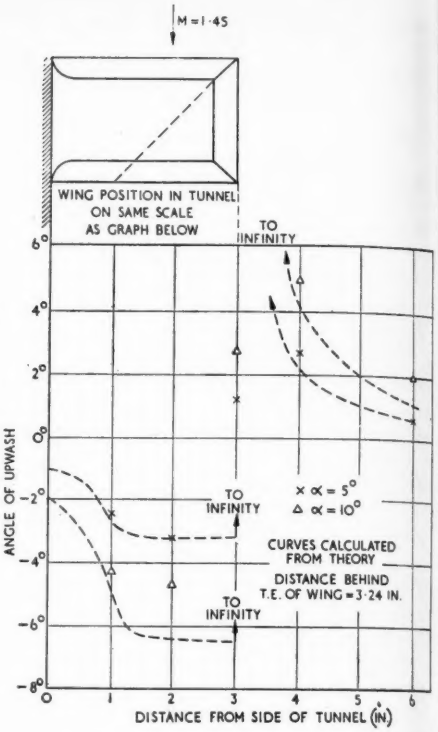

M = 1·45
WING POSITION IN TUNNEL ON SAME SCALE AS GRAPH BELOW
TO INFINITY
TO INFINITY
TO INFINITY
× α = 5°
△ α = 10°
CURVES CALCULATED FROM THEORY
DISTANCE BEHIND T.E. OF WING = 3·24 IN.
ANGLE OF UPWASH
DISTANCE FROM SIDE OF TUNNEL (IN)

Fig. 8.
Spanwise distribution of maximum downwash behind rectangular wing.

change-over region behind the wing tip itself. These results are shown in Figs. 5 and 6.

It will be noticed that the curves for $\alpha = 0°$ follow closely the "no aerofoil" curve in most of the cases shown, this agreement being an indication of the reliability of the experiments. The balance results indicate that the nominal incidences of 0°, 5° and 10° should really be increased by 0.44° to allow for the misalignment of the aerofoil with the wind direction at 0°. On making this allowance, even better agreement was obtained between the curves with and without the aerofoil in Figs. 5 and 6.

The balance results (Fig. 9) also show that the effects were linear up to 15° incidence, and there is every reason to expect that the downwash observations, which were limited to 10°, were in the linear region.

The tunnel was shut down between successive alterations of spanwise position of yawmeter traverse. With the yawmeter fixed in one spanwise position, a traverse was made at right angles to a plane of the wing (thickness-wise) without shutting down the tunnel. It will be seen from Figs. 5 and 6 that the downwash and upwash effects die out fairly rapidly with "thickness-wise" distance above or below the wing wake (the plane of the wing). The upwash or downwash is approximately halved by a "thickness-wise" displacement of half a chord from the maximum. The traverse position of the yawmeter designated negative, corresponds to distances above the upper surface of the aerofoil, and positive corresponds to distances below the lower or pressure face of the aerofoil. As might be expected, the maximum upwash or downwash effect is not located exactly behind the

wing, as linearised theory would demand; in fact, a study of Figs. 5 and 6 will show that the downwash itself deflects downwards the position of maximum downwash and deflects upwards the position of maximum upwash. It will also be seen that a fairly close correlation exists between upwash and deflection of the position of maximum upwash above the upper surface, also between downwash and deflection of the position of maximum downwash downwards. The one noticeable exception to this rule is the traverse made close to the tunnel wall for the rectangular aerofoil.

Sufficient readings were taken for the raked wing tip to enable contours of equal downwash to be drawn. These are shown in Figs. 10 and 11. It should be emphasised that these figures do not include the effects of sidewash. The 0° curve above and below the wing-tip undoubtedly corresponds to a strong sidewash. The streamlines are spirals around the wing-tip position.

It should be noted that the tip effects were reflected by the tunnel wall, and interfered with the readings from 0 to 2 in. from the wall under the conditions of test. The effect is small, amounting to a correction to $d\varepsilon/d\alpha$ of -0.08 at one inch from the wall, and -0.01 at 2 in. These are only 0.8° and 0.1° of downwash for 10° incidence, respectively.

COMPARISON WITH THEORY

In order to compare the experimental observations contained in Figs. 5 and 6 with theory, it is useful to read off the maximum and minimum values of upwash ε, and to plot them against spanwise position, rather than to adopt the plotting of Figs. 5 and 6. This has been done in Figs. 7 and 8 from which the spanwise distribution of the maximum upwash and downwash effects can be judged. The theoretical downwash curves behind the rectangular wing are shown in Fig. 8, and show good agreement with the experimental points. These theoretical curves were obtained from Reference 3.

EFFECT OF VISCOSITY ON THEORETICAL DOWNWASH DISTRIBUTION

In practice the sharp peaks in the downwash distribution will be rounded off by viscous effects, but these are not large, as will now be shown. The case of a simple compressible viscous vortex (subsonic or supersonic) has been solved exactly (*see* for example Reference 4, equation 16.67) to give the circumferential velocity w in the form

$$2\pi rw = K(1 - e^{-r^2/(4\nu t)}).$$

It follows from differentiation of this equation that the radius for maximum velocity is $2\sqrt{(1.256\,\nu t)}$ feet which is of the order of a thousandth of an inch in the present case. It also follows that the maximum velocity is given by

$$w_{max} = \frac{0.319}{\sqrt{(\nu t)}} \frac{K}{2\pi} = \frac{0.319}{\sqrt{(\nu t)}} \frac{L}{2\pi\rho V}$$

where L=lift per unit span of wing, so that $L=\rho V K$.

Dividing by V gives $\varepsilon_{max}=w_{max}/V$ and differentiating with respect to α

$$\left(\frac{d\varepsilon}{d\alpha}\right)_{max} = \frac{0.319}{\sqrt{(\nu t)}} \frac{dC_L}{d\alpha} \frac{c}{4\pi}.$$

The precise age of the vortex from the rectangular wing is uncertain to the extent of

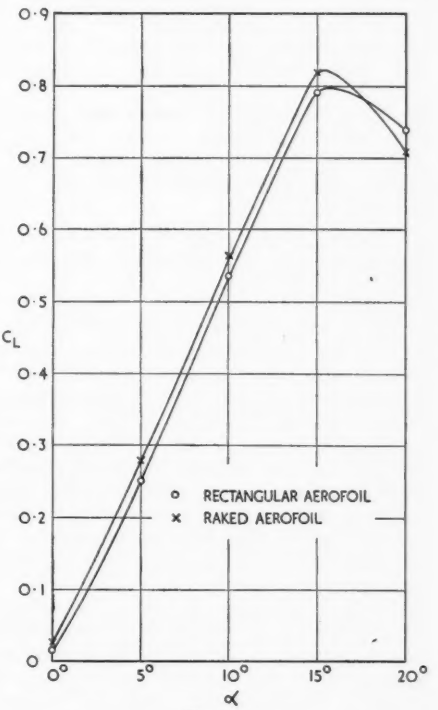


Fig. 9.
Wing lift as measured on electronic balance.

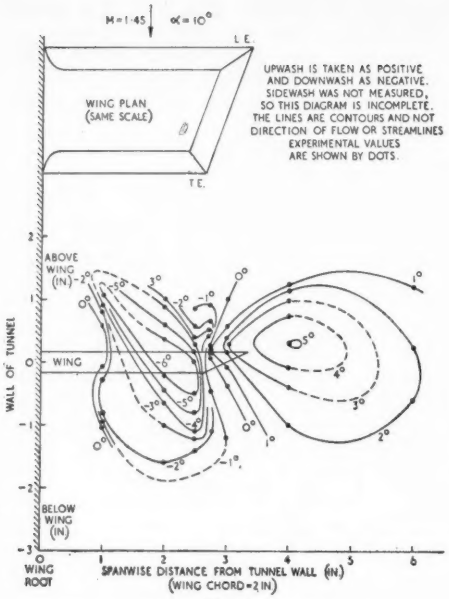

M=1·45
α=10°
L.E.
T.E.
WING PLAN
(SAME SCALE)
UPWASH IS TAKEN AS POSITIVE AND DOWNWASH AS NEGATIVE. SIDEWASH WAS NOT MEASURED, SO THIS DIAGRAM IS INCOMPLETE. THE LINES ARE CONTOURS AND NOT DIRECTION OF FLOW OR STREAMLINES EXPERIMENTAL VALUES ARE SHOWN BY DOTS.
ABOVE WING (IN.)
WING
BELOW WING (IN.)
WALL OF TUNNEL
WING ROOT
SPANWISE DISTANCE FROM TUNNEL WALL (IN.)
(WING CHORD=2 IN.)

Fig. 10.
Contours of equal downwash 1.62 chords behind raked wing tip at 10° incidence.

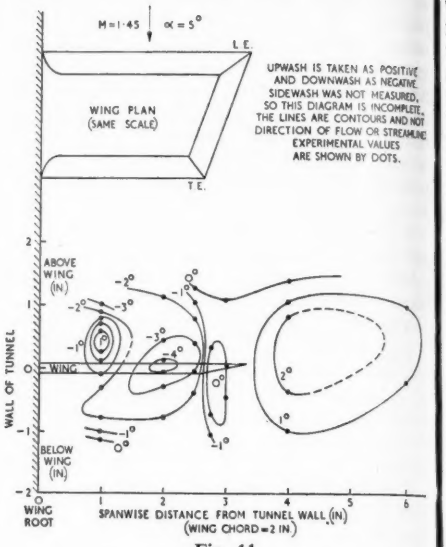

M=1·45
α=5°
L.E.
T.E.
WING PLAN
(SAME SCALE)
UPWASH IS TAKEN AS POSITIVE AND DOWNWASH AS NEGATIVE. SIDEWASH WAS NOT MEASURED, SO THIS DIAGRAM IS INCOMPLETE. THE LINES ARE CONTOURS AND NOT DIRECTION OF FLOW OR STREAMLINES EXPERIMENTAL VALUES ARE SHOWN BY DOTS.
ABOVE WING (IN.)
WING
BELOW WING (IN.)
WALL OF TUNNEL
WING ROOT
SPANWISE DISTANCE FROM TUNNEL WALL (IN.)
(WING CHORD=2 IN.)

Fig. 11.
Contours of equal downwash 1.62 chords behind raked wing tips at 5° incidence.

the wing chord. If the vortex comes from the leading edge of the wing, then $t=(5.04/12)$ ft./(1,400 ft./sec.), and for the trailing edge case $t=(3.04/12)/1{,}400$ sec., and $c=1/6$ ft.

Hence $(d\varepsilon/d\alpha)_{max}$ is between 0.70 and 0.90 respectively. The observed values are a little less than this.

EFFECT OF INDUCED VELOCITIES

In order to calculate supersonic downwash at all, it is necessary to over-simplify the assumptions, by neglecting all non-linear terms in the equations of motion. Among many other things, linear theory considers that the induced velocity field cannot displace the flow bodily. In practice, these induced velocities act so as to twist the flow into a spiral. This twisting effect was in fact observed in the traverse 2.75 in. from the tunnel wall behind the raked wing at 10°. The traverse shows a downwash separated from an upwash by a "thickness-wise" distance of $0.15c$. The theoretical separation of the vortices is $0.35c$ in a spanwise direction, on linearised theory. This effect is well shown by Figs. 10 and 11.

CONCLUSIONS

The main conclusion is the existence of a maximum supersonic downwash $d\varepsilon/d\alpha$ of approximately 0.5 behind a finite wing, with an upwash of similar magnitude in the plane of the wing, outboard of the wing tip.

These effects die out fairly rapidly in a direction perpendicular to the plane of the wing and in any case are confined to the Mach cone from the wing tip. Wing plan form does not appear to have a significant effect on downwash, except in the region immediately behind the wing tip. In this tip region a rectangular wing gives a large upwash just outside the tip, corresponding to the theoretical infinite value, and a constant downwash of $-2\alpha/\pi$ just inside the tip, both from theory and experiments at 5° incidence (*see* Fig. 8).

Raked wing theory shows an infinite upwash behind the outer tip, and an infinite downwash behind the inner tip, as indicated in Fig. 7. The observations seem to fit these facts fairly well, giving a region of "shear" between the two tips, with downwash angle changing rapidly with spanwise distance.

The results described in this paper have direct application to the type of aircraft in which a wing surface is placed ahead of a fixed tail surface. The inner parts of the tail surface will be in a downwash region, and the longitudinal stability produced by this part of the tail therefore will be reduced. Those parts of the tail surface outboard of the wing tip will make a large contribution to longitudinal stability, out of proportion to their area. This area should be made as large as possible.

It is gratifying to note that these rough experiments indicate that supersonic linear downwash theory may be used with reasonable confidence.

ACKNOWLEDGMENT

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THE UNIT OF HEAT

THE Council of the Royal Society has recommended that quantities of heat and all other dependent concepts such as specific heat, latent heat, heat of combustion, entropy and so on, should be expressed in future both in joules and in calories. This practice will be adopted by the publications of the Royal Society and its use has been recommended to all Fellows of the Royal Society.

The attention of all members of the Royal Aeronautical Society is drawn to this recommendation, which it is proposed to adopt for the JOURNAL and the AERONAUTICAL QUARTERLY. The following tables of conversion factors, examples of its use in textual, tabular and diagrammatical matter and the memorandum on "The Unit of Heat", have been prepared by the Royal Society:—

CONVERSION FACTORS FOR VARIOUS CALORIES

Description of calorie	*Value in joules*	*Reciprocal of value in joules*
International steam table calorie (I.T.cal.)	4.1868	0.23885
15° calorie ($cal._{15°}$)	4.1855	0.23892
4° calorie ($cal._{4°}$)	4.2045	0.23784
Thermochemical calorie or defined calorie	4.1840	0.23901
Mean 0° to 100°C. calorie	4.1897	0.23868

CONVERSION FACTORS FOR VARIOUS BRITISH THERMAL UNITS

Description of unit	*Value in joules*	*Reciprocal of value in joules*
60.5°F. British thermal unit	1054.54	0.00094828
Mean 32° to 212°F. British thermal unit	1055.79	0.00094716
39°F. British thermal unit	1059.52	0.00094383
Steam table British thermal unit (1939)	1055.06	0.00094781

EXAMPLES

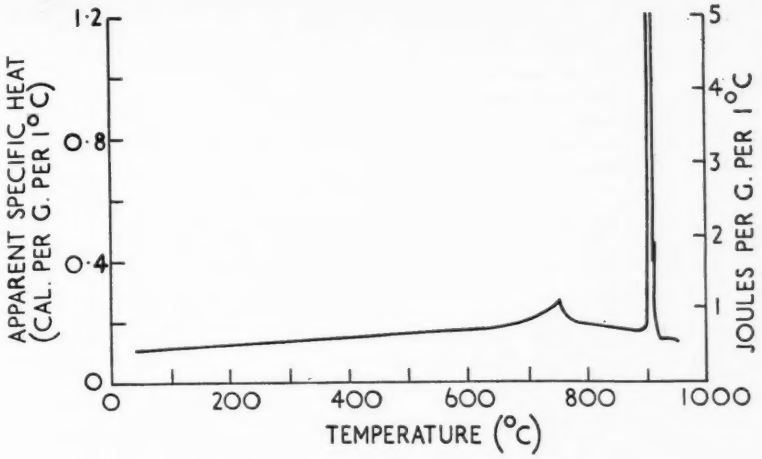
1. Textual

Heat of combustion of carbon monoxide in oxygen is 67.56 $kcal._{15°}$ per mole (282.8 kJ per mole).

2. Tabular

Author	*Date*	*Heat of formation of N_2O (kcal. per mole)*	*(kJ per mole)*
Berthelot	1880	−20.6	−86.2
Thomsen	1882	−17.7	−74.1
Sutton	1932	−20.5	−85.8
Fenning and Cotton	1933	−19.74	−82.6
Awbery and Griffiths	1933	−19.5	−81.6

3. Graphical



MEMORANDUM ON THE UNIT OF HEAT

By Sir CHARLES DARWIN, F.R.S., *Chairman of the Symbols Committee*

1. It has recently been agreed by all the important international bodies concerned that the joule, of 10^7 ergs, is to be the *basic* unit of heat. This is a decided matter, and there can be no doubt of the correctness of the decision. The reasons are briefly as follows.

The First Law of Thermodynamics states that heat is energy, and it is therefore proper that it should be measured in the same units as energy, that is the joule. Hitherto the unit has been the calorie, which was chosen rather arbitrarily more than a century ago for its practical convenience, because the early work in calorimetry was mostly done by observing the change of temperature of a water bath. Thus the calorie involved the idea of temperature which is quite irrelevant to the First Law, and it also had reference to an arbitrarily chosen specific substance, water. To use the calorie it was thus necessary to have a number, the mechanical equivalent of heat, which masqueraded as a universal physical constant, though in fact it is nothing but the specific heat of water measured in energy units.

2. The next point to note is that there are quite a number of different calories in use, differing perceptibly in magnitude, and so making a lot of confusion. The most usual is perhaps (1) the 15° calorie (from 14½° to 15½°), and very nearly but not quite equal to (2) the International Steam Table calorie. Then there is (3) the 4° calorie (which has the advantage of giving the specific heat at constant volume, since it is at the point of maximum density of water), (4) the Thermochemical or 'Defined' calorie, and (5) the Mean calorie (from 0° to 100°). The differences between these range over one part in two hundred. There has also in the past been a minor source of confusion because some writers used the term calorie for the 'Great Calorie', (of a kilogramme instead of a gramme); this should be, and now usually is, called the kilocalorie.

There is a very similar state of confusion over the British Thermal Unit, based on the pound of water, and the degree Fahrenheit, where there are four slightly discrepant definitions also. There has also been defined the quite different unit of the 'pound-calorie', using the centigrade scale, but this has had little vogue and need not be considered. The B.Th.U. happens to be only about 5 per cent. different from the kilojoule.

3. Most calorimetric work has no very high accuracy, and in such work these differences between the calories have no importance. Work of high precision is now practically always done by electrical heating, and the heat is measured by the product of the volts and amperes used and the seconds of time for which the current flows. The mechanical equivalents of the electrical units are known to an accuracy far beyond that attainable in any calorimetric work, so that the results are given automatically directly in joules. To express these results in calories, it is necessary to choose arbitrarily one among the variously defined calories and multiply by the appropriate factor.

4. It must be recognised that the replacement of the calorie by the joule would have effects far outside pure calorimetry, and would have wide repercussions in the whole field of physical chemistry. It would affect not only specific heat, latent heat, and heat of combustion, but also entropy, internal energy, enthalpy, free energy, Gibbs function and thermal conductivity. It may be noticed that for some of these the work is often not calorimetric; for instance many free energies are determined by the voltage of an electrolytic cell, so that here again the determination would be more naturally done directly in joules.

5. The fact that the joule is the basic unit does not necessarily imply that it has got to be used in actual practice, but it would be the natural thing to do, and as has been said, there are a good many cases where it is the quantity actually determined by experiment. The theoretical advantages are overwhelmingly in its favour but the contrary arguments must also be considered. The only really strong argument against it is that it is always inconvenient to make any change of units. Nearly all work in the past has been expressed in calories, and the numerical values, if now given in joules, will be unfamiliar to most people. The process would have the same sort of inconvenience as that of converting British units of mass and length into metric, but it would be far less troublesome, because there is only a single multiplying factor, instead of complicated products of various powers of mass and length, which are needed for the various mechanical quantities. However, this point cannot be invoked as an argument in favour of the joule, since we are forced to convert from British to metric system in order to be comprehensible to foreigners, whereas the calorie is as much used abroad as at home.

6. If the line is taken that the change to the practical use of the joule is too inconvenient to be considered, there is still to be faced the variety among the different calories. In the present instructions given by the International Unions of Physics and Chemistry it is laid down that, where values are given in calories, the conversion factor to joules is always to be stated. This makes a formally self-consistent system, but it is certainly not convenient. A better solution on these lines might be to select a particular value, and define the calorie as, say, exactly 4.187 joules. A difficulty about such a proposal is that it would have to be agreed first by the leading workers (some of whom have vested interests in particular values) and then by the International Unions, and to judge by past experience it might easily take ten years before such agreement would be reached. And in the end after all this we should be left with a unit that was meaningless. Moreover, as pointed out above, much work in fact comes out directly in joules and would have to be converted from the correct into the meaningless unit.

7. It is here contended that as some reform is necessary, it had better be the full reform. Most people naturally fear a change of units, but the fear is exaggerated, as witness the speed with which we can get used to foreign currencies when we are abroad. The present conversion is one of the simplest possible, far simpler than the change from British units to metric, since it only involves one instead of a multiplicity of factors.

8. The aim of the present proposal is to get the joule into common use in numerical work with as little inconvenience as possible. It is proposed that where the numerical value of a thermodynamic quantity is given in a scientific paper it should be given in joules, kilojoules, etc., but that the author may give the value in calories in parentheses afterwards as well. This is somewhat parallel to the method of giving metric equivalents to British mechanical units, which is used in some journals. The proposal recognises that there are at present few who really think in terms of joules, so that it is important that the reader should be helped by also seeing quantities in familiar form. On the other hand, it must be recognised that, unless some drive is put into getting people accustomed to the joule, it will never get into use at all, and we shall be left either with the present confused system, or else with an arbitrary unnatural unit, the only merit of which is that of tradition. It is suggested that the Royal Society is the appropriate body to provide the drive in favour of the correct unit.

9. As to the field over which the proposal should apply, it is intended to be only for strictly scientific work, where there is likely to be little effect on the general public. For example, it is to be presumed that the B.Th.U. will continue to be used in the sale of gas or coal and for the more practical studies of fuels. Again the use of the calorie by dieticians has got into the public consciousness, and it would be inadvisable to change it. Other similar cases may arise.

10. The proposal was intended to apply to the publications of the Royal Society. It is to be hoped that other scientific journals will adopt some similar policy, and this one may be recommended to them, but it must obviously be for them to decide their own policy in the matter, and no exact uniformity is essential.

REVIEWS

AIRCRAFT ENGINE MAINTENANCE AND SERVICE. Rollen H. Drake. The Macmillan Company, New York. 1950. 237 pp. 219 illust. 37s. 6d. net.

This book brings before laymen, students, teachers and licensed aircraft engineers, the fundamentals of aircraft maintenance and service. In simple language, exceedingly well illustrated with photographs and drawings, it will be of great interest to the layman and qualified technician alike, the use of formulae and graphs having been omitted.

Written by an American and based exclusively on American aircraft piston engines and their maintenance, it will be most useful to British service mechanics and students and will provide valuable information for those young men who are now training as Flight Mechanics and Fitters in the Royal Air Force, in view of the fact that they may be called upon to service American aircraft which are now being flown in this country.

The book is divided into sections which deal individually with all aspects of aero-engine maintenance and service. The first half is concerned chiefly with flight maintenance and the inspection of the power plant. There is also a chapter which covers the rectification of engine defects. The second half describes in detail the general procedure for a major overhaul on an engine that has been returned from service.

Inspection procedure such as daily, periodic and major overhaul inspections, as well as propeller maintenance and service, is dealt with exhaustively even so far as to include the methods and working limits necessary in the repair of nicks and such like in both wooden and metallic propeller blades.

Ample space has been given to carburation and running faults due to incorrect mixture, and this should prove most helpful to the service mechanic, as it is written in a clear and concise manner which reveals an intimate knowledge of the subject.

In comparison with the detailed manner in which the engine has been dealt with, it is regretted that only three quarters of a page has been written regarding the exhaust-driven turbo-supercharger and regulator, the engine-driven supercharger having been omitted altogether. This is a pity as it is felt that a chapter, with illustrations, on the turbo-supercharger and the engine-driven supercharger would have been welcomed by both layman and technician, and would have given better balance to the book.

The presentation of the volume is excellent.

PRINCIPLES OF AIRCRAFT PROPULSION MACHINERY. Israel Katz. Pitman Publishing Corporation, New York. 1949. 477 pp. Diagrams. Photographs. Index. $6.50 net.

This book forms the third and most recent addition to the publisher's "Aeronautical Engineering Series." The author says in his preface "The purpose of this book is to inform persons interested in aviation regarding the fundamental aspects and nature of aircraft propulsion machinery, to help them evaluate the real worth of existing equipment in the light of historical data and current development, and to contribute towards the advancement of aviation as a popular and reliable means of transportation." To this end the author divides his book into eight parts, under the headings Orientation, Development, Fundamental Aspects, Performance, Piston Engines, Gas Turbines, Advanced Power Plants, and Application. Each part in turn consists of a number of short chapters which, except for those included in Parts 5, 6 and 7, attempt to cover both piston engines and turbines.

In a volume of 477 pp., such a task is impossible, except on the most general lines and by exercising very severe compression. Inevitably there must be some risk of superficiality and sketchiness, irritating to the serious student and misleading to the inexperienced. In fairness to the author, however, it should be said that he intends his book to serve mainly as reference material and a guide to further reading, and with these objects he gives useful bibliographies and a number of "problems" at the end of each chapter. The book is plentifully illustrated.

Chapter I contains a useful classification of engine types; Chapters II and III describe current high- and low-power engines in terms too often reminiscent of a manufacturer's sales brochure; and Chapter IV describes some current turbine power plants. In Part 2, Chapter V is briefly historical. Chapter VI reviews development (the knighthood bestowed on the Director, N.G.T.E.—p. 73—may be assumed to be intelligent anticipation) and Chapters VII and VIII deal respectively with current developments and future prospects. Part 3 contains chapters on thermodynamic principles, pulse jets, piston-engine cycles, continuous combustion cycles, fuels and combustion (Fig. 15.3—an alleged typical "combustor"—is so erroneous as to be completely misleading).

Part 4 deals with performance criteria, power availability, operating characteristics, economy, response to controls, and maintenance. Part 5 covers the enormous field of piston engine components, fixed and moving, valves, accessories, lubrication, intake and exhaust systems, fuel and ignition systems, cooling systems, controls and operating techniques in eleven brief chapters. Part 6 covers a smaller field for the gas turbine. Part 7 (one chapter) deals with rocket motors, and Part 8 with installation, testing, and the choice of power plants.

Although the book is unavoidably sketchy, and in the main descriptive, it may perhaps be recommended to advanced students as a guide to further reading.

REFUELLING IN FLIGHT. C. H. Latimer-Needham, M.Sc., F.R.Ae.S. Pitman. London. 1950. 200 pp. Figures. Diagrams. Bibliography. 12s. 6d. net.

Those who saw the exhibition of flight refuelling at the S.B.A.C. show in September, and many who did not, cannot fail to be interested in a book giving the history, technique and possibilities of this achievement. There can be no more authoritative book than this, written by the Chief Engineer of Flight Refuelling Ltd., and it is, as well, an interesting and readable one.

The advantages of flight refuelling, both from the civil and military aspect, are presented in an easily understandable and convincing way. The calculations of payload, range and optimum refuelling points are thorough, without seeming laborious and the results are presented in a series of clear and simple diagrams. We see, for example, that with properly planned flight-refuelled services the range of air liners need but seldom exceed 3,000 still-air miles; that for the same payload an air liner operating non-stop between London and New York would weigh three times as much as one taking advantage of flight refuelling, and would cost nearly four times as much. With a light weight take-off, the take-off run can be reduced by about 20 per cent., and the aircraft may be refuelled in flight to an overload weight, in excess of that allowable for the actual take-off. The reduction in weight and size made possible by refuelling in flight has a direct bearing on aerodrome size, and it is shown that aerodrome cost is approximately proportional to the square of the runway length. The safety aspect is considered—the reduction of fire hazard, and the elimination of a number of take-offs and landings (during which 70 per cent. of accidents occur).

The cost of flight refuelling is discussed, but there is little mention of other disadvantages and difficulties, and it seems surprising that a development which obviously has so much to offer to aviation should have so few drawbacks. Certainly a good many of the technical snags have been ironed out in the 16 years of research and development work done by Flight Refuelling Ltd. This work has led to the highly efficient "probe-and-drogue" method seen at Farnborough, in which up to 300 gallons of fuel a minute can be delivered, and the whole refuelling operation for

a fighter takes two to three minutes. Later developments cannot be divulged for security reasons.

The production of the book is of a high standard; and the tables and diagrams, which are clear and simple, are well arranged with respect to the corresponding text. The Appendices contain details of the equipment, and there is an extensive list of references.

This book will be of interest to the general reader, and of considerable use to those who wish to study the subject in more detail.

PROTECTIVE COATINGS FOR METALS. J. W. Gailer and E. J. Vaughan. Charles Griffin & Co. Ltd. London. 1950. 261 pp. Figures. Tables. Appendix. Index. 24s. net.

Man, a metallurgist of more than 5,000 years' experience, in spite of his keen observation, lively imagination and enthusiasm for experiment, has been slow and not particularly successful in discovering means for protecting his hard-won metals. Paint he certainly knew from beyond the dawn of history, but his use of it was merely as a gratification of his growing aestheticism. Metallic protection by such a means is but a relatively new achievement—an affair of less than two hundred years, while the employment of alternative types of film for the same purpose, has yet to celebrate its centenary.

Such a subject, with its clear historical development and obvious current importance, gives scope for a book which could and should be both stimulating and informative, yet the authors of the one under review have not so succeeded. Both are Masters of Science, yet neither would seem to be a master of bookcraft. Their introduction might so well have been a broad, inspiring evolutionary tale carrying one on, with interest and expectation, to technical descriptions of various protective processes. In fact, it is so dull and stilted that its brevity constitutes its merits. There is no outline of current theory of metallic corrosion, no table of relative electro-potentials of the commercial metals and alloys, no appropriate stressing of the vast importance of contact corrosion—indeed, there is little *raison d'être* for much of what follows.

The chapters dealing with the various methods of forming protective films amount to eleven. They cover an immense field and, as a logical consequence, lack depth. One recalls Gardner's immense and erudite tome on paints, and notes that our authors are able to cover, in some respects, a somewhat wider field in a mere 13 pages. One reads what they have to say with regard to anodising and then re-reads the D.T.D.910B Specification. The latter, in most respects, is both more lucid and more informative. With magnesium alloys in mind, one applies the same formula. One finds that, by comparison, the anonymous author of D.T.D.911A deserves a measure of praise which, in all probability, he has never received. Where this book covers the same ground as the D.T.D. Specifications 901-911, members of the Aircraft Industry will find the latter the more profitable reading. This work may have a public, but clearly not one composed of readers of this JOURNAL.

CORRESPONDENCE

In "Some Notes on the terms '*g*' and Inertia" (Graduates' and Students' Section Inset, November 1950 JOURNAL), Mr. Campion states in Part III, paragraph (*v*), that because the flight speed is assumed unchanged when the pilot moves the elevator controls, the additional tail load P will act at the aerodynamic centre of the tailplane and *the tailplane couple M_T will be unchanged.*

Unfortunately this is only true when the elevator controls are not connected to the elevator! Obviously any change in elevator angle will result in a change in M_T, since

$$M_T = \tfrac{1}{2}\rho S' c' V^2 m \eta$$

where $m = (dC_{MT})/(d\eta)$

η = elevator angle

S' = tailplane area

c' = tailplane mean chord

It should also be realised that the "Flight cases involving Pitching Accelerations," as laid down in current B.C.A.R.'s and A.P.970, are empirical and should be used with caution, especially on high speed aircraft which are really beyond the scope of such empirical rules.

KEITH J. OLDHAM (*Graduate* R.Ae.S.)

MR. CAMPION'S REPLY

I am grateful to Mr. Oldham for drawing attention to this error.

The second paragraph of Part III, paragraph (*v*), should be amended to read:—

"Then for instantaneous equilibrium:

$$L_W + L_T + P = n'W \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$M_0 + L_W (h - h_0)\,\bar{c} + (M_T + \Delta M_T) - (P + L_T)\,l_T = \alpha I_p \quad . \quad . \quad . \quad (14)$$

where L_W, L_T, M_o and M_T have the same values as in equations (11) and (12), and ΔM_T is that total change in M_T due to the change in elevator angle. Since the total vertical load has changed, the normal acceleration will no longer be n, but will be n', and as the inertia factor is numerically equal to the acceleration in "g's," the new normal factor is

$$n' = (L_W + L_T + P)/W = n + P/W \quad . \quad . \quad . \quad . \quad (15)$$

The angular inertia factor in pitch is given by

$$\alpha = [M_0 + L_W (h - h_0)\,\bar{c} + (M_T + \Delta M_T) - (P + L_T)\,l_T]/I_p = [\Delta M_T - P l_T]/W \quad . \quad (16)"$$

It should be noted that paragraph (*vi*), which described the loads occurring in gust cases, is not affected by this correction, since the change of load is not due to a change of elevator angle.

The warning contained in the final paragraph of Mr. Oldham's letter is premature to some extent, since Part IV of the paper includes some comments on the differences between a rigid aircraft and one whose structure is regarded as flexible. In a paper of this nature, it is impossible to delve deeply into the problems of the design of high speed aircraft, and Mr. Oldham's warning should be heeded.

Since structural design and aerodynamics are becoming increasingly interdependent, I should like to conclude with an appeal to those Graduates and Students

who have some knowledge and experience of this relationship, which can be called "the domain of aeroelasticity." There is little information available to the student of aeronautics which explains the basic principles of aeroelasticity and flutter in elementary terms, and if a series of papers could be written by, and for, Graduate and Student Members, this lack would be overcome. The Graduates' and Students' Section's Honorary Editor, Mr. M. K. Bowden, would be pleased to hear from any member who is willing to write such a paper for the Section's pages of the JOURNAL.

M. C. CAMPION (*Graduate*)

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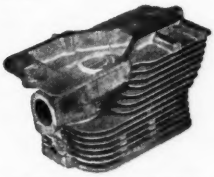

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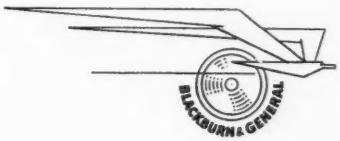

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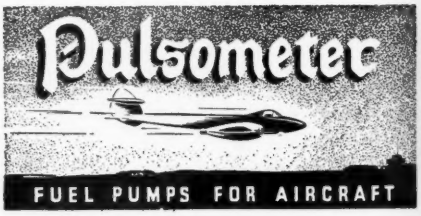

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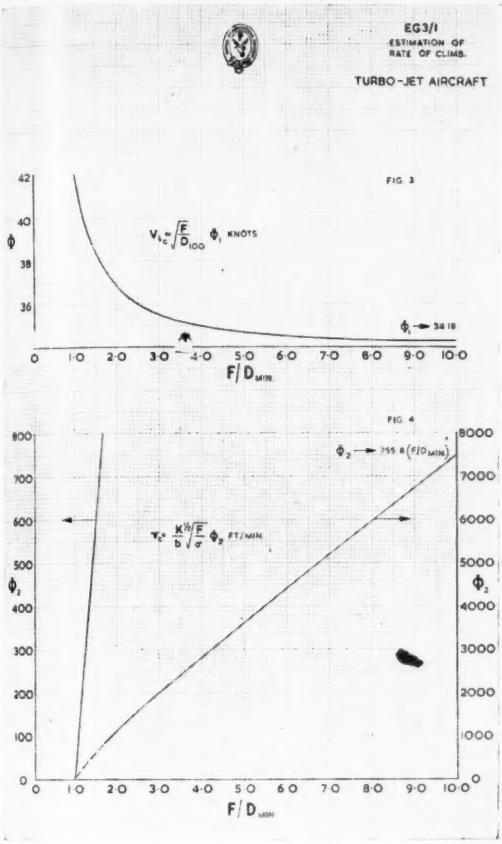

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